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Animal Intelligence

An Experimental Study of the Associative Processes in Animals

BY

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ANIMAL INTELLIGENCE: AN EXPERIMENTAL STUDY OF THE ASSOCIATIVE PROCESSES IN ANIMALS.

This monograph is an attempt at an explanation of the nature of the process of association in the animal mind. Inasmuch as there have been no extended researches of a character similar to the present one either in subject-matter or experimental method, it is necessary to explain briefly its standpoint.

Our knowledge of the mental life of animals equals in the main our knowledge of their sense-powers, of their instincts or reactions performed without experience, and of their reactions which are built up by experience. Confining our attention to the latter we find it the opinion of the better observers and analysts that these reactions can all be explained by the ordinary associative processes without aid from abstract, conceptual, inferential thinking. These associative processes then, as present in animals' minds and as displayed in their acts, are my subject-matter. Any one familiar in even a general way with the literature of comparative psychology will recall that this part of the field has received faulty and unsuccessful treatment. The careful, minute, and solid knowledge of the sense-organs of animals finds no counterpart in the realm of associations and habits. We do not know how delicate or how complex or how permanent are the possible associations of any given group of animals. And although one would be rash who said that our present equipment of facts about instincts was sufficient or that our theories about it were surely sound, yet our notion of what occurs when a chick grabs a worm are luminous and infallible compared to our notion of what happens when a kitten runs into the house at the familiar call. The reason that they have satisfied us as well as they have is just that they are so vague. We say that the kitten associates the sound 'kitty kitty' with the experience

of nice milk to drink, which does very well for a common-sense answer. It also suffices as a rebuke to those who would have the kitten ratiocinate about the matter, but it fails to tell what real mental content is present. Does the kitten feel "*sound of call, memory-image of milk in a saucer in the kitchen, thought of running into the house, a feeling, finally, of 'I will run in'?*" Does he perhaps feel only the sound of the bell and an impulse to run in, similar in quality to the impulses which make a tennis player run to and fro when playing? The word association may cover a multitude of essentially different processes, and when a writer attributes anything that an animal may do to association his statement has only the negative value of eliminating reasoning on the one hand and instinct on the other. His position is like that of a zoölogist who should to-day class an animal among the 'worms.' To give to the word a positive value and several definite possibilities of meaning is one aim of this investigation.

The importance to comparative psychology in general of a more scientific account of the association-process in animals is evident. Apart from the desirability of knowing all the facts we can, of whatever sort, there is the especial consideration that these associations and consequent habits have an immediate import for biological science. In the higher animals the bodily life and preservative acts are largely directed by these associations. They, and not instinct, make the animal use the best feeding grounds, sleep in the same lair, avoid new dangers and profit by new changes in nature. Their higher development in mammals is a chief factor in the supremacy of that group. This, however, is a minor consideration. The main purpose of the study of the animal mind is to learn the development of mental life down through the phylum, to trace in particular the origin of human faculty. In relation to this chief purpose of comparative psychology the associative processes assume a rôle predominant over that of sense-powers or instinct, for in a study of the associative processes lies the solution of the problem. Sense-powers and instincts have changed by addition and supersedence, but the cognitive side of consciousness has changed not only in quantity but also in quality. Somehow

out of these associative processes have arisen human consciousnesses with their sciences and arts and religions. The association of ideas proper, imagination, memory, abstraction, generalization, judgment, inference, have here their source. And in the metamorphosis the instincts, impulses, emotions and sense-impressions have been transformed out of their old natures. For the origin and development of human faculty we must look to these processes of association in lower animals. Not only then does this department need treatment more, but promises to repay the worker better.

Although no work done in this field is enough like the present investigation to require an account of its results, the *method* hitherto in use invites comparison by its contrast and, as I believe, by its faults. In the first place, most of the books do not give us a psychology, but rather a *eulogy*, of animals. They have all been about animal *intelligence*, never about animal *stupidity*. Though a writer derides the notion that animals have reason, he hastens to add that they have marvellous capacity of forming associations, and is likely to refer to the fact that human beings only rarely reason anything out, that their trains of ideas are ruled mostly by association, as if, in this latter, animals were on a par with them. The history of books on animals' minds thus furnishes an illustration of the well-nigh universal tendency in human nature to find the marvelous wherever it can. We wonder that the stars are so big and so far apart, that the microbes are so small and so thick together, and for much the same reason wonder at the things animals do. They used to be wonderful because of the mysterious, God-given faculty of instinct, which could almost remove mountains. More lately they have been wondered at because of their marvellous mental powers in profiting by experience. Now imagine an astronomer tremendously eager to prove the stars as big as possible, or a bacteriologist whose great scientific desire is to demonstrate the microbes to be very, very little! Yet there has been a similar eagerness on the part of many recent writers on animal psychology to praise the abilities of animals. It cannot help leading to partiality in deductions from facts and more especially in the choice of facts

for investigation. How can scientists who write like lawyers, defending animals against the charge of having no power of rationality, be at the same time impartial judges on the bench? Unfortunately the real work in this field has been done in this spirit. The level-headed thinkers who might have won valuable results have contented themselves with arguing against the theories of the eulogists. They have not made investigations of their own.

In the second place the facts have generally been derived from anecdotes. Now quite apart from such pedantry as insists that a man's word about a scientific fact is worthless unless he is a trained scientist, there are really in this field special objections to the acceptance of the testimony about animals' intelligent acts which one gets from anecdotes. Such testimony is by no means on a par with testimony about the size of a fish or the migration of birds, etc. For here one has to deal not merely with ignorant or inaccurate testimony, but also with prejudiced testimony. Human folk are as a matter of fact eager to find intelligence in animals. They like to. And when the animal observed is a pet belonging to them or their friends, or when the story is one that has been told as a story to entertain, further complications are introduced. Nor is this all. Besides commonly misstating what fact sthey report, they report only such facts as show the animal at his best. Dogs get lost hundreds of times and no one ever notices it or sends an account of it to a scientific magazine. But let one find his way from Brooklyn to Yonkers and the fact immediately becomes a circulating anecdote. Thousands of cats on thousands of occasions sit helplessly yowling, and no one takes thought of it or writes to his friend, the professor; but let one cat claw at the knob of a door supposedly as a signal to be let out, and straightway this cat becomes the representative of the cat-mind in all the books. The unconscious distortion of the facts is almost harmless compared to the unconscious neglect of an animal's mental life until it verges on the unusual and marvelous. It is as if some denizen of a planet where communication was by thought-transference, who was surveying humankind and reporting their psychology, should be oblivious to all our

inter-communication save such as the psychical-research society has noted. If he should further misinterpret the cases of mere coincidence of thoughts as facts comparable to telepathic communication, he would not be more wrong than some of the animal psychologists. In short, the anecdotes give really the *abnormal* or *super-normal* psychology of animals.

Further, it must be confessed that these vices have been only ameliorated, not obliterated, when the observation is first-hand, is made by the psychologist himself. For as men of the utmost scientific skill have failed to prove good observers in the field of spiritualistic phenomena,[1] so biologists and psychologists before the pet terrier or hunted fox often become like Samson shorn. They, too, have looked for the intelligent and unusual and neglected the stupid and normal.

Finally, in all cases, whether of direct observation or report by good observers or bad, there have been three other defects. Only a single case is studied, and so the results are not necessarily true of the type; the observation is not repeated, nor are the conditions perfectly regulated; the previous history of the animal in question is not known. Such observations may tell us, if the observer is perfectly reliable, that a certain thing takes place, but they cannot assure us that it will take place universally among the animals of that species, or universally with the same animal. Nor can the influence of previous experience be estimated. All this refers to means of getting knowledge about what animals *do*. The next question is, "What do they *feel?*" Previous work has not furnished an answer or the material for an answer to this more important question. Nothing but carefully designed, crucial experiments can. In abandoning the old method one ought to seek above all to replace it by one which will not only tell more accurately *what they do*, and give the much-needed information *how they do it*, but also inform us *what they feel* while they act.

To remedy these defects experiment must be substituted for

[1] I do not mean that scientists have been too credulous with regard to spiritualism, but am referring to the cases where ten or twenty scientists have been sent to observe some trick-performance by a spiritualistic 'medium,' and have all been absolutely confident that they understood the secret of its performance, *each of them giving a totally different explanation.*

observation and the collection of anecdotes. Thus you immediately get rid of several of them. You can repeat the conditions at will, so as to see whether or not the animal's behavior is due to mere coincidence. A number of animals can be subjected to the same test, so as to attain typical results. The animal may be put in situations where its conduct is especially instructive. After considerable preliminary observation of animals' behavior under various conditions, I chose for my general method one which, simple as it is, possesses several other marked advantages besides those which accompany experiment of any sort. It was merely to put animals when hungry in enclosures from which they could escape by some simple act, such as pulling at a loop of cord, pressing a lever, or stepping on a platform. (A detailed description of these boxes and pens will be given later.) The animal was put in the enclosure, food was left outside in sight, and his actions observed. Besides recording his general behavior, special notice was taken of how he succeeded in doing the necessary act (in case he did succeed), and a record was kept of the time that he was in the box before performing the successful pull, or clawing, or bite. This was repeated until the animal had formed a perfect association between the sense-impression of the interior of that box and the impulse leading to the successful movement. When the association was thus perfect, the time taken to escape was, of course, practically constant and very short.

If, on the other hand, after a certain time the animal did not succeed, he was taken out, but *not fed*. If, after a sufficient number of trials, he failed to get out, the case was recorded as one of complete failure. Enough different sorts of methods of escape were tried to make it fairly sure that association in general, not association of a particular sort of impulse, was being studied. Enough animals were taken with each box or pen to make it sure that the results were not due to individual peculiarities. None of the animals used had any previous acquaintance with any of the mechanical contrivances by which the doors were opened. So far as possible the animals were kept in a uniform state of hunger, which was practically utter hunger. That is, no cat or dog was experimented on when the experi-

ment involved any important question of fact or theory, unless I was sure that his motive was of the standard strength. With chicks this is not practicable, on account of their delicacy. But with them dislike of loneliness acts as a uniform motive to get back to the other chicks. Cats (or rather kittens), dogs and chicks were the subjects of the experiments. All were apparently in excellent health, save an occasional chick.

By this method of experimentation the animals are put in situations which call into activity their mental functions and permit them to be carefully observed. One may, by following it, observe personally more intelligent acts than are included in any anecdotal collection. And this actual vision of animals in the act of using their minds is far more fruitful than any amount of histories of what animals have done without the history of how they did it. But besides affording this opportunity for purposeful and systematic observation, our method is valuable because it frees the animal from any influence of the observer. The animal's behavior is quite independent of any factors save its own hunger, the mechanism of the box it is in, the food outside, and such general matters as fatigue, indisposition, etc. Therefore the work done by one investigator may be repeated and verified or modified by another. No personal factor is present save in the observation and interpretation. Again, our method gives some very important results which are quite uninfluenced by *any* personal factor in any way. The curves showing the progress of the formation of associations, which are obtained from the records of the times taken by the animal in successive trials, are facts which may be obtained by any observer who can tell time. They are absolute, and whatever can be deduced from them is sure. So also the question of whether an animal does or does not form a certain association requires for an answer no higher qualification in the observer than a pair of eyes. The literature of animal psychology shows so uniformly and often so sadly the influence of the personal equation that any method which can partially eliminate it deserves a trial.

Furthermore, although the associations formed are such as could not have been previously experienced or provided for by heredity, they are still not too remote from the animal's ordinary

course of life. They mean simply the connection of a certain act with a certain situation and resultant pleasure, and this general type of association is found throughout the animal's life normally. The muscular movements required are all such as might often be required of the animal. And yet it will be noted that the acts required are nearly enough like the acts of the anecdotes to enable one to compare the results of experiment by this method with the work of the anecdote school. Finally, it may be noticed that the method lends itself readily to experiments on imitation.

We may now start in with the description of the apparatus and of the behavior of the animals.[1]

Description of Apparatus.

The shape and general apparatus of the boxes which were used for the cats is shown by the accompaning drawing of box K. Unless special figures are given, it should be understood

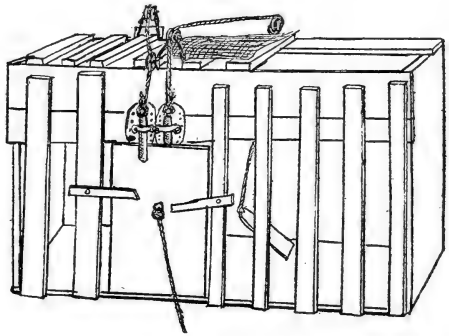

Fig. 1.

that each box is approximately 20 inches long by 15 broad by 12 high. Except where mention is made to the contrary, the door was pulled open by a weight attached to a string which ran over a pulley and was fastened to the door, just as soon as the

[1] The experiments now to be described were for the most part made in the Psychological Laboratory of Columbia University during the year '97–'98, but a few of them were made in connection with a general preliminary investigation of animal psychology undertaken at Harvard University in the previous year.

animal loosened the bolt or bar which held it. Especial care was taken not to have the widest openings between the bars at all near the lever, or wire-loop, or what not, which governed the bolt on the door. For the animal instinctively attacks the large openings first, and if the mechanism which governs the opening of the door is situated near one of them the animal's task is rendered easier. You do not then get the association process so free from the helping hand of instinct as you do if you make the box without reference to the position of the mechanism to be set up within it. These various mechanisms are so simple that a verbal description will suffice in most cases. The facts which the reader should note are the nature of the movement which the cat had to make, the nature of the object at which the movement was directed, and the position of the object in the box. In some special cases attention will also be called to the force required. In general, however, that was very slight (20 to 100 grams if applied directly). The various boxes will be designated by capital letters.

A. A string attached to the bolt which held the door ran up over a pulley on the front edge of the box, and was tied to a wire loop (2½ inches diameter) hanging 6 inches above the floor in front center of box. Clawing or biting it, or rubbing against it even, if in a certain way, opened the door. We may call this box A '*O at front*.'

B. A string attached to the bolt ran up over a pulley on the front edge of the door, then across the box to another pulley screwed into the inside of the back of the box 1¼ inches below the top, and passing over it ended in a wire loop (3 inches in diameter) 6 inches above the floor in back center of box. Force applied to the loop or *to the string* as it ran across the top of the box between two bars would open the door. We may call B '*O at back*.'

B1. In B1 the string ran outside the box, coming down through a hole at the back, and was therefore inaccessible and invisible from within. Only by pulling the loop could the door be opened. B1 may be called '*O at back 2nd*.'

C. A door of the usual position and size (as in Fig. 1) was kept closed by a wooden button 3½ inches long, ⅞ inch wide,

½ inch thick. This turned on a nail driven into the box ½ inch above the middle of the top edge of the door. The door would fall inward as soon as the button was turned from its vertical to a horizontal position. A pull of 125 grams would do this if applied sideways at the lowest point of the button 2¼ inches below its pivot. The cats usually clawed the button round by downward pressure on its top edge, which was 1¼ inches above the nail. Then, of course, more force was necessary. C may be called '*Button*.'

D. The door was in the extreme right of the front. A string fastened to the bolt which held it ran up over a pulley on the top edge and back to the top edge of the back side of the box (3 inches in from the right side) and was there firmly fastened. The top of the box was of wire screening and arched over the string ¾ inch above it along its entire length. A slight pull on the string anywhere opened the door. This box was 20 × 16, but a space 7 × 16 was partitioned off at the left by a wire screen. D may be called '*String*.'

D1 was the same box as B, but had the string fastened firmly at the back instead of running over a pulley and ending in a wire loop. We may call it '*String 2nd*.'

E. A string ran from the bolt holding the door up over a pulley and down to the floor outside the box, where it was fastened 2 inches in front of the box and 1½ inches to the left of the door (looking from the inside). By poking a paw out between the bars and pulling this string inward the door would be opened. We may call E '*String outside*.'

In F the string was not fastened to the floor but ended in a loop 2½ inches in diameter which could be clawed down so as to open the door. Unless the pull was in just the right direction, the string was likely to catch on the pulley. This loop hung 3 inches above the floor, and 1¾ inches in front of the box. We may call F '*String outside unfastened*.'

G was a box 29 × 20½ × 22½, with a door 29 × 12 hinged on the left side of the box (looking from within), and kept closed by an ordinary thumb-latch placed 15 inches from the floor. The remainder of the front of the box was closed in by wooden bars. The door was a wooden frame covered with screening.

It was *not* arranged so as to open as soon as the latch was lifted, but required a force of 400 grams, even when applied to the best advantage. The bar of the thumb-latch, moreover, would fall back into place again unless the door were pushed out at least a little. The top of this box was not of bars or screening, but solid. We may call G '*Thumb-latch*.'

H was, except for the opening where the door was situated, a perfectly solid and dark box. In the front was cut an opening about 9 × 7 inches. A wooden frame covered with wire netting hung in front of this. It was fastened to the box only by a screw through the middle of the frame's top piece, and could therefore be pushed to either side so as to permit escape from the box if it were prevented from swinging back into place.

I was a box 12 × 14 × 23. The door was 8 inches wide, 4 high, and hinged at the left side. It was held closed by a wooden bar which moved easily on a pivot and which could be pushed up by another bar which projected 5 inches into the box. This second bar was pivoted so that downward pressure on it pushed the first bar up and let the door swing open. The second bar entered the box at a point 4 inches above the floor and 2½ inches in from the right side of the box. In its normal position its inner end was 5½ inches above the floor. A depression of 2 inches at that end was necessary to open the door. Of course, nearer the pivot a shorter depression would do. The front of the box was closed by bars, but the rest by solid boards. We may call I '*Lever*.'

J was the same as B except that the door was not opened by a weight as soon as the bolt was pulled up. On the contrary, the door was held closed by a small piece of board (4 × 3½ × ¾ thick) placed against it outside. After pulling the loop at the back the cat had to knock down this support and push the door open. We may call J '*Double*.'

K was a box arranged so that three separate acts were required to open the door, which was held by two bolts at the top and two bars outside. One of the bolts was connected with a platform in the back center of the box so that depressing the platform raised the bolt. The other was raised by a string

which ran up over a pulley in the front, across the box 1 inch above the bars, over a pulley near the corner of the box, and down to the floor, where it was fastened. Pulling on this string, either by clawing at it where it was running vertically from the last pulley to the floor, or by putting the paw out between the bars which covered the top of the box, and clawing the string downwards, would raise the bolt. If both bolts were raised and *either* bar was pushed up or down far enough to be out of the way, the cat could escape. K, or 'Triple,' as it may be called, is the box reproduced in Figure 1.

L was a box that also required three acts to open the door. It was a combination of A (O at front), D (string), I (lever). The lever or bar to be depressed was 2 inches to the right of the door, which was in the front center. The string to be clawed or bitten ran from front center to back center 1 inch below the top of the box.

Z was a box with back and sides entirely closed, with front and top closed by bars and screening, with a small opening in the left front corner. A box was held in front of this and drawn away when the cats happened to lick themselves. Thus escape and food followed always upon the impulse to lick themselves, and they soon would immediately start doing so as soon as pushed into the box. The same box was used with the impulse changed to that for scratching themselves. The size of this box was $15 \times 10 \times 16$.

Experiments with Cats.

In these various boxes were put cats from among the following. I give approximately their ages while under experiment.

No. 1. 8–10 months.
No. 2. 5–7 months.
No. 3. 5–11 months.
No. 4. 5–8 months.
No. 5. 5–7 months.
No. 6. 3–5 months.
No. 7. 3–5 months.
No. 8. 6–6½ months.
No. 10. 4–8 months.

No. 11. 7–8 months.
No. 12. 4–6 months.
No. 13. 18–19 months.

The behavior of all but 11 and 13 was practically the same. When put into the box the cat would show evident signs of discomfort and of an impulse to escape from confinement. It tries to squeeze through any opening; it claws and bites at the bars or wire; it thrusts its paws out through any opening and claws at everything it reaches; it continues its efforts when it strikes anything loose and shaky; it may claw at things within the box. It does not pay very much attention to the food outside, but seems simply to strive instinctively to escape from confinement. The vigor with which it struggles is extraordinary. For eight or ten minutes it will claw and bite and squeeze incessantly. With 13, an old cat, and 11, an uncommonly sluggish cat, the behavior was different. They did not struggle vigorously or continually. On some occasions they did not even struggle at all. It was therefore necessary to let them out of some box a few times, feeding them each time. After they thus associate climbing out of the box with getting food, they will try to get out whenever put in. They do not, even then, struggle so vigorously or get so excited as the rest. In either case, whether the impulse to struggle be due to an instinctive reaction to confinement or to an association, it is likely to succeed in letting the cat out of the box. The cat that is clawing all over the box in her impulsive struggle will probably claw the string or loop or button so as to open the door. And gradually all the other non-successful impulses will be stamped out and the particular impulse leading to the successful act will be stamped in by the resulting pleasure, until, after many trials, the cat will, when put in the box, immediately claw the button or loop in a definite way.

The starting point for the formation of any association in these cases, then, is the set of instinctive activities which are aroused when a cat feels discomfort in the box either because of confinement or a desire for food. This discomfort, plus the sense-impression of a surrounding, confining wall, expresses itself prior to any experience, in squeezings, clawings, bitings,

etc. From among these movements one is selected by success. But this is the starting point only in the case of the first box experienced. After that the cat has associated with the feeling of confinement certain impulses which have led to success more than others and are thereby strengthened. A cat that has learned to escape from A by clawing has when put into C or G a greater tendency to claw at things than it instinctively had at the start, and a less tendency to squeeze through holes. A very pleasant form of this decrease in instinctive impulses was noticed in the gradual cessation of howling and mewing. However, the useless instinctive impulses die out slowly, and often play an important part even after the cat has had experience with six or eight boxes. And what is important in our previous statement, namely, that the activity of an animal when first put into a new box is not directed by any appreciation of *that* box's character, but by certain general impulses to acts, is not affected by this modification. Most of this activity is determined by heredity; some of it, by previous experience.

My use of the words *instinctive* and *impulse* may cause some misunderstanding unless explained here. Let us, throughout this book, understand by instinct any reaction which an animal makes to a situation *without experience*. It thus includes unconscious as well as conscious acts. Any reaction, then, to totally new phenomena, when first experienced, will be called instinctive. Any impulse then felt will be called an instinctive impulse. Instincts include whatever the nervous system of an animal, as far as inherited, is capable of. My use of the word will, I hope, everywhere make clear what fact I mean. If the reader gets the fact meant in mind it does not in the least matter whether he would himself call such a fact instinct or not. Any one who objects to the word may substitute 'hocus-pocus' for it wherever it occurs. The definition here made will not be used to prove or disprove any theory, but simply as a signal for the reader to imagine a certain sort of fact.

The word *impulse* is used against the writer's will, but there is no better. Its meaning will probably become clear as the reader finds it in actual use, but to avoid misconception at any time I will state now that *impulse* means the consciousness ac-

companying a muscular innervation *apart* from *that feeling of the act which comes from seeing oneself move, from feeling one's body in a different position, etc.* It is the *direct feeling of the doing* as distinguished from the *idea of the act done* gained through eye, etc. For this reason I say 'impulse *and* act' instead of simply 'act.' Above all, it must be borne in mind that by impulse I never mean the *motive* to the act. In popular speech you may say that hunger is the impulse which makes the cat claw. That will never be the use here. The word *motive* will always denote that sort of consciousness. Any one who thinks that the act ought not to be thus subdivided into impulse and deed may feel free to use the word *act* for *impulse* or *impulse and act* throughout, if he will remember that the act in this aspect of being felt as to be done or as doing is in animals the important thing, is the thing which gets associated, while the act as done, as viewed from outside, is a secondary affair. I prefer to have a separate word, impulse, for the former, and keep the word act for the latter, which it commonly means.

Starting, then, with its store of instinctive impulses, the cat hits upon the successful movement, and gradually associates it with the sense-impression of the interior of the box until the connection is perfect, so that it performs the act as soon as confronted with the sense-impression. The formation of each association may be represented graphically by a time-curve. In these curves lengths of one millimeter along the abscissa represent successive experiences in the box, and heights of one millimeter above it each represent ten seconds of time. The curve is formed by joining the tops of perpendiculars erected along the abscissa 1 mm. apart (the first perpendicular coinciding with the y line), each perpendicular representing the time the cat was in the box before escaping. Thus, in Fig. 2 on page 18 the curve marked 12 in A shows that, in 24 experiences or trials in box A, cat 12 took the following times to perform the act, 160 sec., 30 sec., 90 sec., 60, 15, 28, 20, 30, 22, 11, 15, 20, 12, 10, 14, 10, 8, 8, 5, 10, 8, 6, 6, 7. A short vertical line below the abscissa denotes that an interval of approximately 24 hours elapsed before the next trial. Where the interval was longer it is designated by a figure 2 for two days, 3 for three

days, etc. If the interval was shorter the number of hours is specified by 1 hr., 2 hrs., etc. In many cases the animal failed in some trial to perform the act in ten or fifteen minutes and was then taken out by me. Such failures are denoted by a break in the curve either at its start or along its course. In some cases there are short curves after the main ones. These, as shown by the figures beneath, represent the animal's mastery of the association after a very long interval of time, and may be called memory curves. A discussion of them will come in the last part of the book.

The time-curve is obviously a fair representation of the progress of the formation of the association, for the two essential factors in the latter are the disappearance of all activity save the particular sort which brings success with it, and perfection of that particular sort of act so that it is done precisely and at will. Of these the second is, on deeper analysis, found to be a part of the first; any clawing at a loop except the particular claw which depresses it is theoretically a useless activity. If we stick to the looser phraseology, however, no harm will be done. The combination of these two factors is inversely proportional to the time taken, provided the animal surely wants to get out at once. This was rendered almost certain by the degree of hunger. Theoretically a perfect association is formed when both factors are perfect,—when the animal, for example, does nothing but claw at the loop, and claws at it in the most useful way for the purpose. In some cases (*c. g.*, 2 in K on page 26) neither factor ever gets perfected in a great many trials. In some cases the first factor does but the second does not, and the cat goes at the thing not always in the desirable way. In all cases there is a fraction of the time which represents getting oneself together after being dropped in the box, and realizing where one is. But for our purpose all these matters count little, and we may take the general slope of the curve as representing very fairly the progress of the association. The slope of any particular part of it may be due to accident. Thus, very often the second experience may have a higher time-point than the first, because the first few successes may all be entirely due to accidentally hitting the loop, or whatever it is, and whether the

accident will happen sooner in one trial than another is then a matter of chance. Considering the general slope, it is, of course, apparent that a gradual descent—say, from initial times of 300 sec. to a constant time of 6 or 8 sec. in the course of 20 to 30 trials—represents a difficult association, while an abrupt descent, say in 5 trials, from a similar initial height, represents a very easy association. Thus, 2 in Z, on page 23, is a hard, and 1 in I, on page 22, an easy association.

In boxes A, C, D, E, I, 100 % of the cats given a chance to do so hit upon the movement and formed the association. The following table shows the results where some cats failed.

	No. cats tried.	*No. cats failed.*
F	5	4
G	8	5
H	9	2
J	5	2
K	5	2

The time-curves follow. By referring to the description of apparatus they will be easily understood. Each mm. along the abscissa represents one trial. Each mm. above it represents 10 seconds.

These time-curves show, in the first place, what associations are easy for an animal to form, and what are hard. The act must be one which the animal will perform in the course of the activity which its inherited equipment incites or its previous experience has connected with the sense-impression of a box's interior. The oftener the act naturally occurs in the course of such activity, the sooner it will be performed in the first trial or so, and this is one condition, sometimes, of the ease of forming the association. For if the first few successes are five minutes apart the influence of one may nearly wear off before the next, while if they are forty seconds apart the influences may get summated. But this is not the only or the main condition of the celerity with which an association may be formed. It depends also on the amount of attention given to the act. An act of

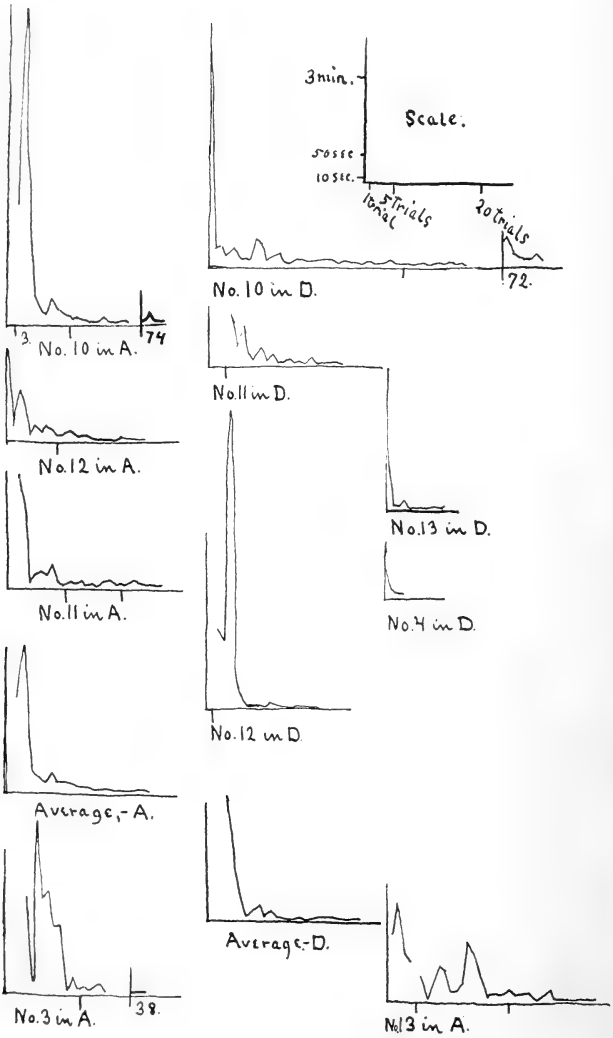
3 min.
Scale.
50 sec.
10 sec.
1 trial
5 trials
20 trials
No. 10 in D.
72.
3.
No. 10 in A.
74
No. 11 in D.
No. 12 in A.
No. 13 in D.
No. 11 in A.
No. 4 in D.
No. 12 in D.
Average, - A.
Average - D.
No. 3 in A.
38.
No. 13 in A.

FIG. 2.

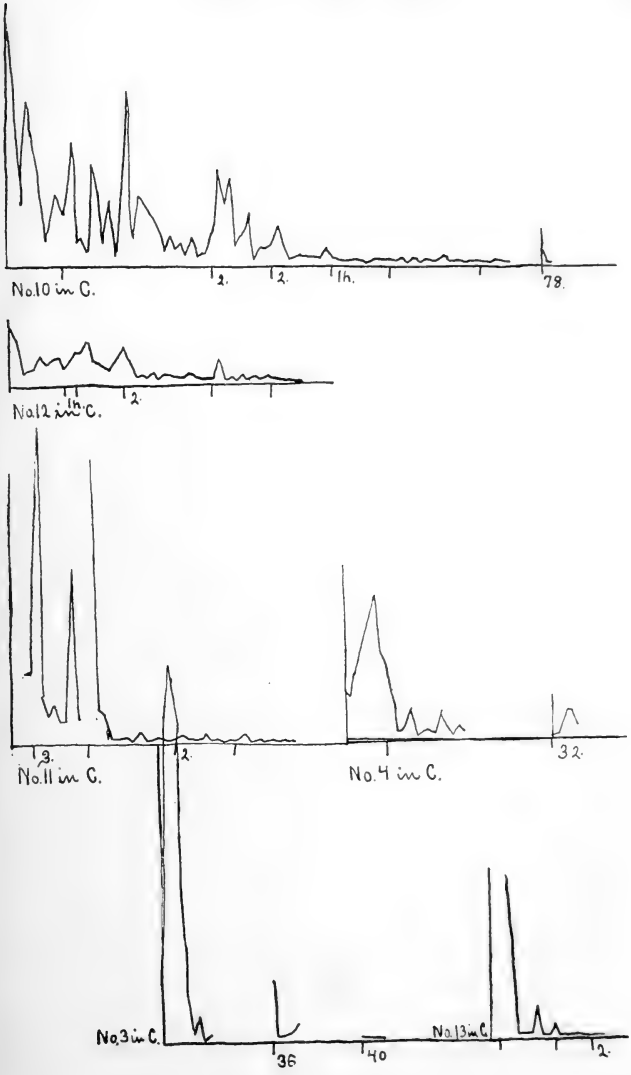
No.10 in C.
2.
2.
1h.
78.
No.12 in C.
1h.
2.
No.11 in C.
3.
2.
No.4 in C.
32.
No.3 in C.
36
40
No.13 in C.
2.

FIG. 3.

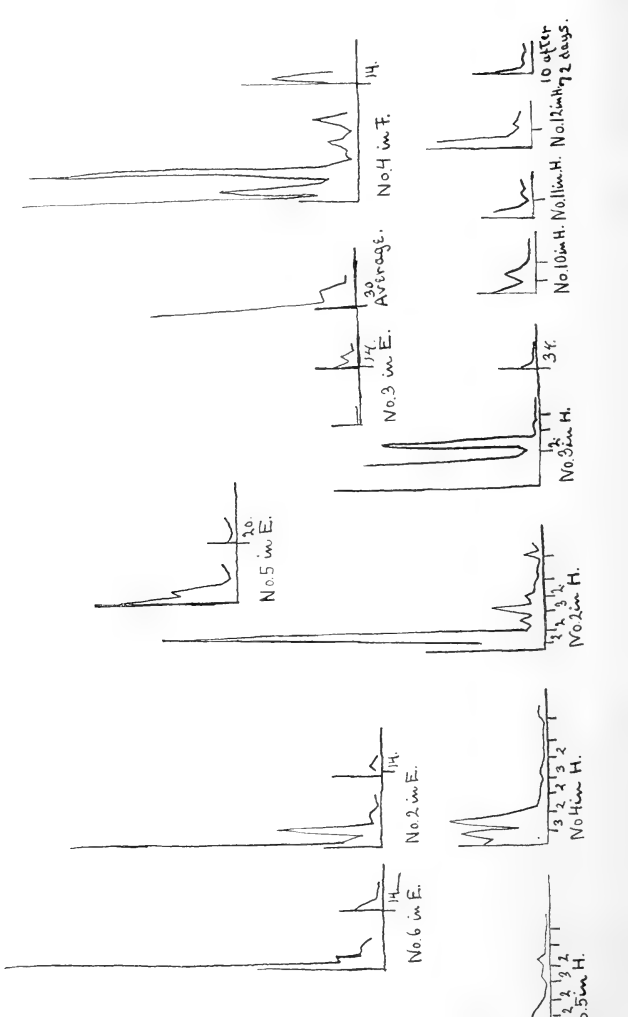
No.5 in H.
No.6 in E.
No.2 in E.
No.4 in H.
No.5 in E.
No.3 in E.
No.4 in F.
No.2 in H.
No.3 in H.
No.10 in H.
No.1 in H.
No.1 in H. 72 days.
10 after

FIG. 4.

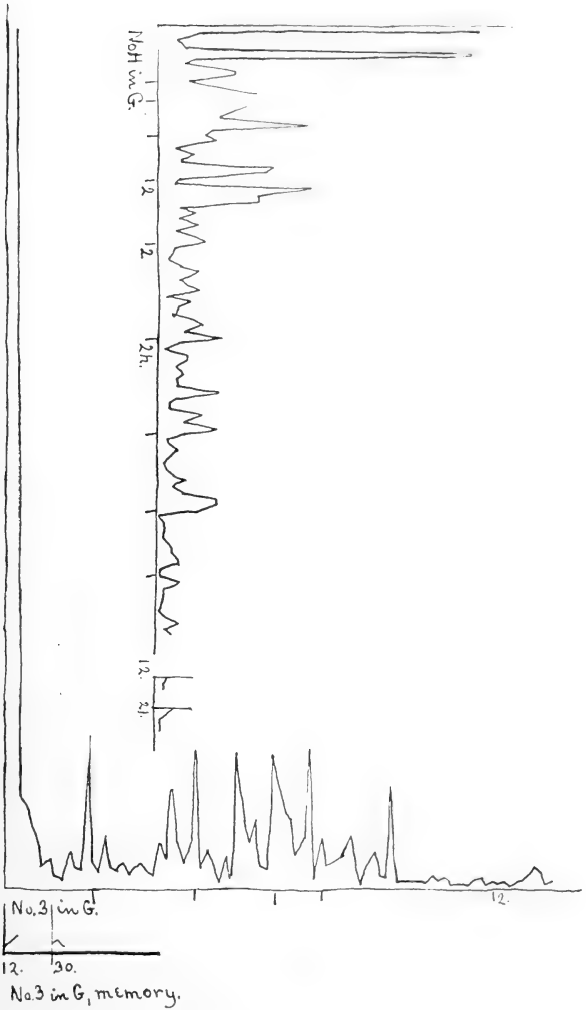

FIG. 5.

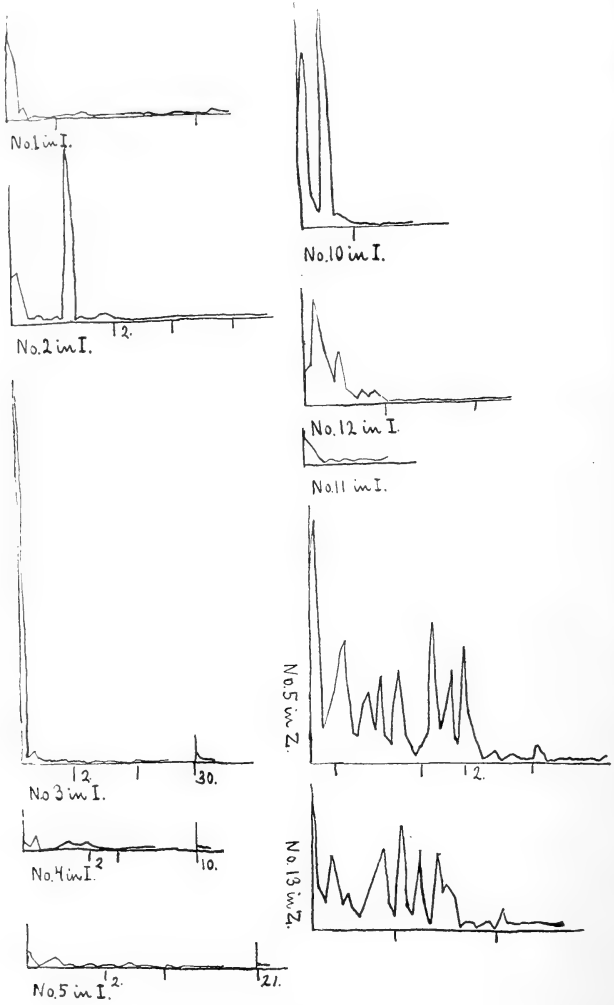
No. 1 in I.
No. 10 in I.
2.
No. 2 in I.
No. 12 in I.
No. 11 in I.
No. 5 in Z.
2.
2
30.
No. 3 in I.
No. 4 in I.
2
10.
No. 13 in Z.
No. 5 in I.
2.
21.

FIG 6

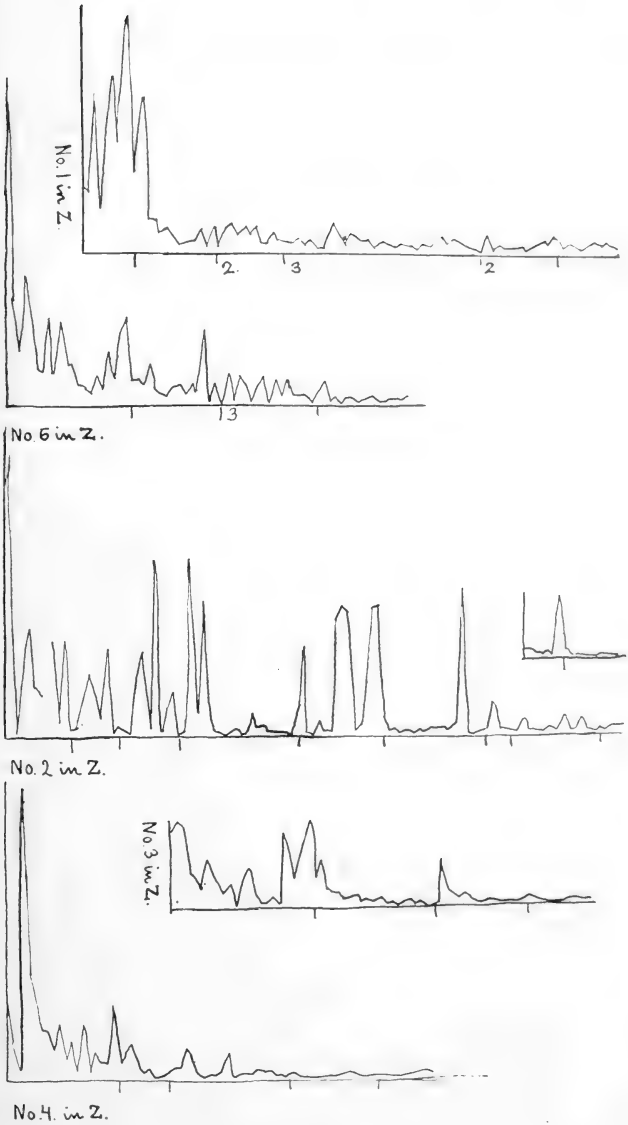

FIG. 7

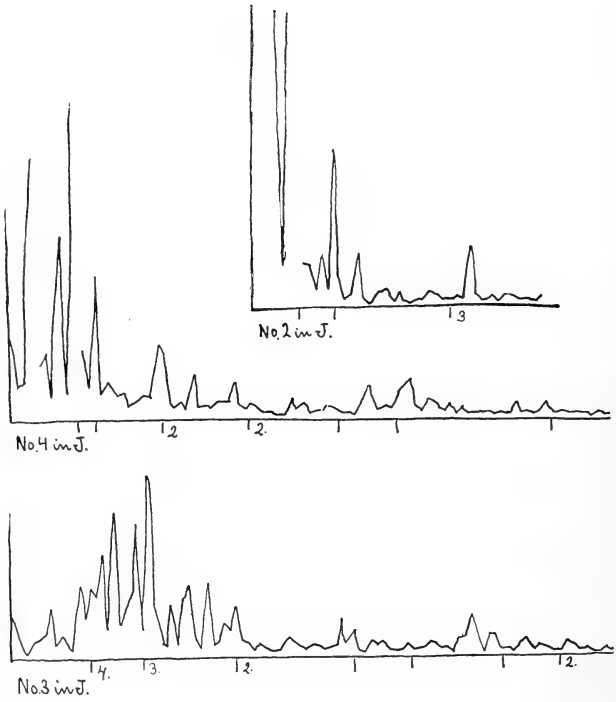

FIG. 8.

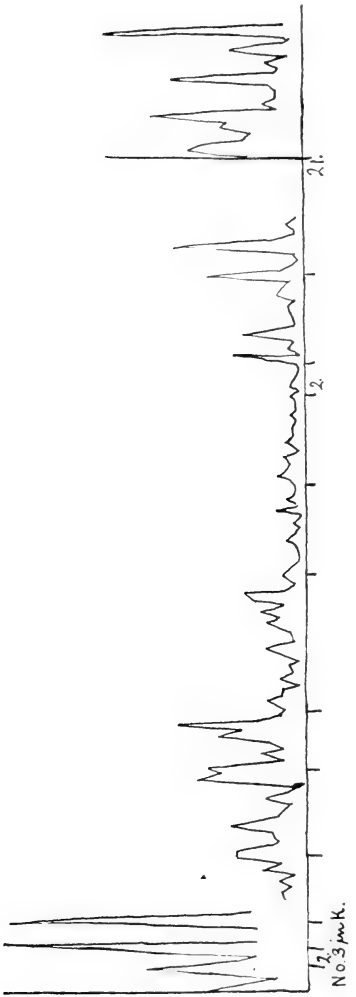

FIG. 9.

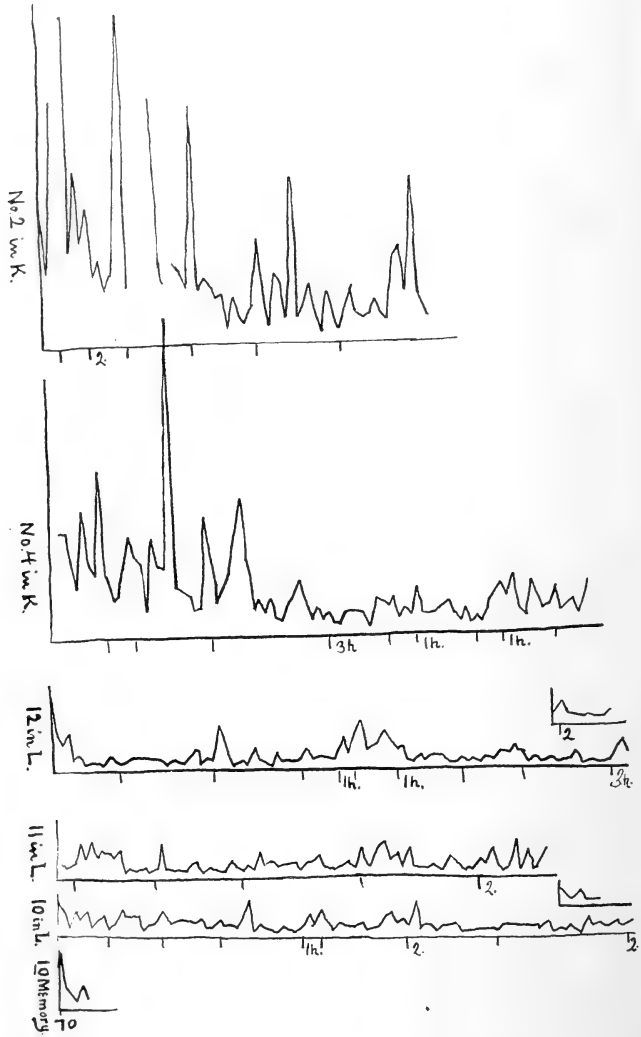
No. 2 in K.
2.
No. 4 in K.
3h.
1h.
1h.
12 in L.
1h.
1h.
2.
2h.
11 in L.
2.
10 in L.
1h.
2.
2
10 Memory
10

FIG. 10.

the sort likely to be well attended to will be learned more quickly. Here, too, accident may play a part, for a cat may merely happen to be attending to its paw when it claws. The kinds of acts which insure attention are those where the movement which works the mechanism is one which the cat makes definitely to get out. Thus A (O at front) is easier to learn than C (button), because the cat does A in trying to claw down the front of the box and so is attending to what it does, whereas it does C generally in a vague scrabble along the front or while trying to claw outside with the other paw, and so does not attend to the little unimportant part of its act which turns the button round. Above all, *simplicity* and *definiteness* in the act make the association easy. G (thumb-latch), J (double) and K and L (triples) are hard, because complex. E is easy, because directly in the line of the instinctive impulse to try to pull oneself out of the box by clawing at anything outside. It is thus very closely attended to. The extreme of ease is reached when a single experience stamps the association in so completely that ever after the act is done at once. This is approached in I and E. In these experiments the sense-impressions offered no difficulty one more than the other.

Vigor, abundance of movements, was observed to make differences between individuals in the same association. It works by shortening the first times, the times when the cat still does the act largely by accident. Nos. 3 and 4 show this throughout. Attention, often correlated with lack of vigor, makes a cat form an association quicker after he gets started. No. 13 shows this somewhat. The absence of a fury of activity let him be more conscious of what he did do.

The curves on pages 22 and 23 showing the history of cats 1, 5, 13 and 3, which were let out of the box Z when they licked themselves, and of cats 6, 2, and 4, which were let out when they scratched themselves, are interesting because they show associations where there is no congruity (no more to a cat than to a man) between the act and the result. One chick, too, was thus freed whenever he pecked at his feathers to dress them. He formed the association, and would whirl his head round and poke it into the feathers as soon as dropped in the box. There

is in all these cases a noticeable tendency, of the cause of which I am ignorant, to diminish the act until it becomes a mere vestige of a lick or scratch. After the cat gets so that it performs the act soon after being put in, it begins to do it less and less vigorously. The licking degenerates into a mere quick turn of the head with one or two motions up and down with tongue extended. Instead of a hearty scratch, the cat waves its paw up and down rapidly for an instant. Moreover, if sometimes you do not let the cat out after this feeble reaction, it does not at once repeat the movement, as it would do if it depressed a thumb-piece, for instance, without success in getting the door open. Of the reason for this difference I am again ignorant.

Previous experience makes a difference in the quickness with which the cat forms the associations. After getting out of six or eight boxes by different sorts of acts the cat's general tendency to claw at loose objects within the box is strengthened and its tendency to squeeze through holes and bite bars is weakened; accordingly it will learn associations along the general line of the old more quickly. Further, its tendency to pay attention to what it is doing gets strengthened, and this is something which may properly be called a change in degree of intelligence. A test was made of the influence of experience in this latter way by putting two groups of cats through I (lever), one group (1, 2, 3, 4, 5) after considerable experience, the other (10, 11, 12) after experience with only one box. As the act in I was not along the line of the acts in previous boxes, and as a decrease in the squeezings and bitings would be of little use in the box as arranged, the influence of experience in the former way was of little account. The curves of all are shown on page 22.

If the whole set of curves are examined in connection with the following table, which gives the general order in which each animal took up the different associations which he eventually formed, many suggestions of the influence of experience will be met with. The results are not exhaustive enough to justify more than the general conclusion that there is such an influence. By taking more individuals and thus eliminating all other factors besides experience, one can easily show just how and how far experience facilitates association.

When in this table the letters designating the boxes are in italics it means that, though the cat formed the association, it was in connection with other experiments and so is not recorded in the curves.

Cat 1	*A*	*B*	*C*	D_1	*D*	Z	I.	
" 2	*C*	D_1	*D*	E	Z	H	J	I K.
" 3	A	C	E	G	H	J	Z	I K.
" 4	C	F	G	D	Z	H	J	I K.
" 5	*C*	E	Z	H	I.			
" 6	*A*	*C*	E	Z.				
" 7	*A*	*C*						
" 10	C	I	A	H	D	L		
" 11	C	I	A	H	D	L		
" 12	C	I	A	H	D	L		
" 13	A	C	D	G	Z			

The advantage due to experience in our experiments is not, however, the same as ordinarily in the case of trained animals. With them the associations are with the acts or voice of man or with sense impressions to which they naturally do not attend (*e.g.*, figures on a blackboard, ringing of a bell, some act of another animal). Here the advantage of experience is mainly due to the fact that by such experience the animals gain the habit of attending to the master's face and voice and acts and to sense impressions in general.

I made no attempt to find the differences in ability to acquire associations due to age or sex or fatigue or circumstances of any sort. By simply finding the average slope in the different cases to be compared, one can easily demonstrate any such differences that exist. So far as this discovery is profitable, investigation along this line ought now to go on without delay, the method being made clear. Of differences due to difference in the species, genus, etc. Of the animals I will speak after reviewing the time-curves of dogs and chicks.

In the present state of animal psychology there is another

value to these results which was especially aimed at by the investigator from the start. They furnish a quantitative estimate of what the average cat can do, so that if any one has an animal which he thinks has shown superior intelligence or perhaps reasoning power, he may test his observations and opinion by taking the time-curves of the animal in such boxes as I have described.[1]

If his animal in a number of cases forms the associations very much more quickly, or deals with the situation in a more intelligent fashion than my cats did, then he may have ground for claiming in his individual a variation toward greater intelligence and possibly intelligence of a different order. On the other hand, if the animal fails to rise above the type in his dealings with the boxes, the observer should confess that his opinion of the animal's intelligence may have been at fault and should look for a correction of it.

We have in these time-curves a fairly adequate measure of what the ordinary cat can do, and how it does it, and in similar curves soon to be presented a less adequate measure of what a dog may do. If other investigators, if especially all amateurs who are interested in animal intelligence, will take other cats and dogs, especially those supposed by owners to be extraordinarily intelligent, and experiment with them in this way, we shall soon get a notion of how much variation there is among animals in the direction of more or superior intelligence. The beginning here made is meager but solid. The knowledge it gives needs to be much extended. The variations found in individuals should be correlated not merely with supposed superiority in intelligence, a factor too vague to be very serviceable, but with observed differences in vigor, attention, memory and muscular skill. No phenomena are more capable of exact and thorough investigation by experiment than the associations of animal consciousness. Never will you get a better psychological subject than a hungry cat. When the crude beginnings of this research have been improved and replaced by more ingen-

[1] To any such person who may chance to read this monograph I may say that I will gladly furnish him photographs of the boxes, so that he may work with exact duplicates of them.

ious and adroit experimenters, the results ought to be very valuable.

Surely every one must agree that no man now has a right to advance theories about what is in animals' minds or to deny previous theories unless he supports his thesis by systematic and extended experiments. My own theories, soon to be proclaimed, will doubtless be opposed by many. I sincerely hope they will, provided the denial is accompanied by actual experimental work. In fact I shall be tempted again and again in the course of this book to defend some theory, dubious enough to my own mind, in the hope of thereby inducing some one to oppose me and in opposing me to make the experiments I have myself had no opportunity to make yet. Probably there will be enough opposition if I confine myself to the theories I feel sure of.

Experiments with Dogs.

The boxes used were as follows:

AA was similar to A (O at front), except that the loop was of stiff cord ⅜ inch in diameter and was larger (3½ inches diameter); also it was hung a foot from the floor and 8 inches to the right of the door. The box itself was 41 × 20 × 23.

BB was similar to B, the loop being the same as in AA, and being hung a foot from the floor. The box was of the same size and shape as AA.

BB1 was like BB, but the loop was hung 18 inches from the floor.

CC was similar to C (Button), but the button was 6 inches long, and the box was 36½ × 22 × 23.

II was similar to I, but the box was 30 × 20 × 25 inches; the door (11 inches wide, 6 high) was in the left front corner, and the lever was 6 inches long and entered the box at a point 2 inches to the right of the door and 4 inches above the floor.

In M the same box as in II was used, but instead of a lever projecting inside the box, a lever running outside parallel to the plane of the front of the box and 18 inches long was used. This lay close against the bars composing the front of the box, and could be pawed down by sticking the paw out an inch or

so between two bars, at a point about 15 inches high and 6 inches in from the right edge of the front. We may call M '*Lever outside.*'

N was a pen 5×3 feet made of wire netting 46 inches high. The door, 31×20, was in the right half of the front. A string from the bolt passed up over a pulley and back to the back center, where it was fastened 33 inches above the floor. Biting or pawing this string opened the door.

O was like K, except that there was only one bar, that the string ran inside the box, so that it was easily accessible, and that the bolt raised in K by depression of the platform could be raised in O (and was by the dog experimented on) by sticking the muzzle out between two bars just above the bolt and by biting the string, at the same time jerking it upward. O was 30 ×20×25 in size.

The box G was used for both dogs and cats, without any variation save that for dogs the resistance of the door to pressure outwards was doubled.

In these boxes were put in the course of the experiments dog 1 (about 8 months old), and dogs 2 and 3, adults, all of small size.

A dog who, when hungry, is shut up in one of these boxes is not nearly so vigorous in his struggles to get out as is the young cat. And even after he has experienced the pleasure of eating on escape many times he does not try to get out so hard as a cat, young or old. He does try to a certain extent. He paws or bites the bars or screening, and tries to squeeze out in a tame sort of way. He gives up his attempts sooner than the cat, if they prove unsuccessful. Furthermore his attention is taken by the food, not the confinement. He wants to get *to* the food, not *out of* the box. So, unlike the cat, he confines his efforts to the front of the box. It was also a practical necessity that the dogs should be kept from howling in the evening, and for this reason I could not use as a motive the utter hunger which the cats were made to suffer. In the morning, when the experiments were made, the dogs were surely hungry, and no experiment is recorded in which the dog was not in a state to be willing to make a great effort for a bit of

meat, but the motive may not have been even and equal throughout, as it was with the cats.

The curves which follow are to be interpreted in the same way as those for the cats, and are on the same scale. The order in which No. 1 took up the various associations was AA, BB, BB1, G, N, CC, II, O.

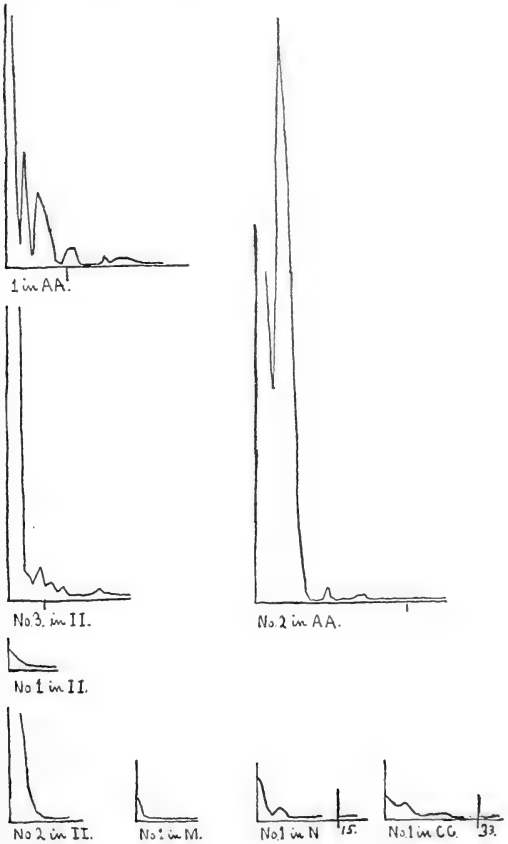

FIG. 11.

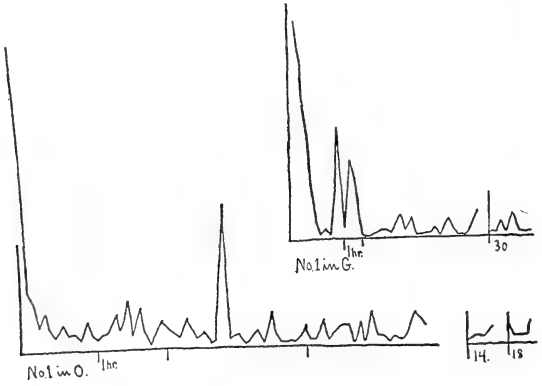

FIG. 12.

The percentage of dogs succeeding in the various boxes is given below, but is of no consequence, because so few were tried, and because the motive, hunger, was not perhaps strong enough, or equal in all cases.

In AA 3 out of 3.
" BB 0 " " 2 (that is, without previous experience of AA).
In CC 1 out of 2.
" II 3 " " 3.
" M 1 " " 2.
" N 1 " " 3.
" G 1 " " 3.

EXPERIMENTS WITH CHICKS.

The apparatus was as follows:

P was simply a small pen arranged with two exits, one leading to the enclosure where were the other chicks and food, one leading to another pen with no exit. The drawing (Fig. 13 on page 35) explains itself. A chick was placed at A and left to find its way out. The walls were made of books stuck up on end.

Q was a similar pen arranged so that the real exit was harder to find. (See Fig. 14.)

R was still another pen similarly constructed, with four possible avenues to be taken. (See Fig. 15.)

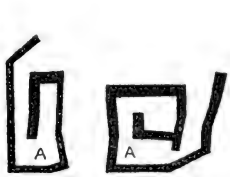

FIG. 13. FIG. 14. FIG. 15.

S was a pen with walls 11 inches high. On the right side an inclined plane of wire screening led from the floor of the pen to the top of its front wall. Thence the chick could jump down to where its fellows and the food and drink were. S was 17 × 14 in size.

T was a pen of the same size as S, with a block of wood 3 inches by 3 and 2 inches high in the right back corner. From this an inclined plane led to the top of the front wall (on the right side of the box). But a partition was placed along the left edge of this plane, so that a chick could reach it only via the wooden block, not by a direct jump.

U was a pen 16 × 14 × 10 inches. Along the back toward the right corner were placed a series of steps 1½ inches wide, the first 1, the second 2, and the third 3 inches high. In the corner was a platform 4 × 4, and 4 high, from which access to the top of the front wall of the pen could be gained by scrambling up inside a stovepipe 11 inches long, inclined upward at an angle of about 30°. From the edge of the wall the chick could of course jump down to food and society. The top of the pen was covered so that the chick could not from the platform jump onto the edge of the stovepipe or the top of the pen wall. The only means of exit was to go up the steps to the platform, up through the stovepipe to the front wall, and then jump down.

The time-curves for chicks 90, 91, 92, 93, 94 and 95, all 2–8 days old when experimented on, follow on page 37. The scale is the same as that in the curves of the cats and dogs.

Besides these simple acts, which any average chick will accidentally hit upon and associate, there are, in the records of my preliminary study of animal intelligence, a multitude of all sorts of associations which some chicks have happened to form. Chicks have escaped from confinement by stepping on a little platform in the back of the box, by jumping up and pulling a string like that in D, by pecking at a door, by climbing up a spiral staircase and out through a hole in the wall, by doing this and then in addition walking across a ladder for a foot to another wall from which they could jump down, etc., etc. Not every chick will happen upon the right way in these cases, but the chicks who did happen upon it all formed the associations perfectly after enough trials.

The behavior of the chicks shows the same general character as that of the cat, conditioned, of course, by the different nature of the instinctive impulses. Take a chick put in T (inclined plane) for an example. When taken from the food and other chicks and dropped into the pen he shows evident signs of discomfort; he runs back and forth, peeping loudly, trying to squeeze through any openings there may be, jumping up to get over the wall, and pecking at the bars or screen, if such separate him from the other chicks. Finally, in his general running around he goes up the inclined plane a way. He may come down again, or he may go on up far enough to see over the top of the wall. If he does, he will probably go running up the rest of the way and jump down. With further trials he gains more and more of an impulse to walk up an inclined plane when he sees it, while the vain running and pecking, etc., are stamped out by the absence of any sequent pleasure. Finally, the chick goes up the plane as soon as put in. In scientific terms this history means that the chick, when confronted by loneliness and confining walls, responds by those acts which in similar conditions in nature would be likely to free him. Some one of these acts leads him to the successful act, and the resulting pleasure stamps it in. Absence of pleasure stamps all others out. The case is just the same as with dogs and cats. The time-curves are shown in Fig. 16.

Coming now to the question of differences in intelligence

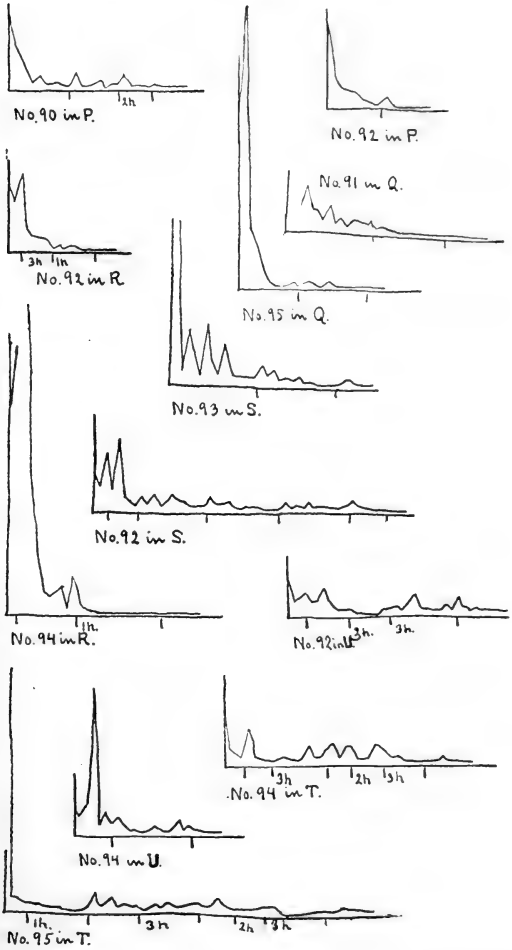

FIG. 16.

between the different animals, it is clear that such differences are hard to estimate accurately. The chicks are surely very

much slower in forming associations and less able to tackle hard ones, but the biggest part of the difference between what they do and what the dogs and cats do is not referable so much to any difference in intelligence as to a difference in their bodily organs and instinctive impulses. As between dogs and cats, the influence of the difference in quantity of activity, in the direction of the instinctive impulses, in the versatility of the fore-limb, is hard to separate from the influence of intelligence proper. The best practical tests to judge such differences in general would be differences in memory, which are very easily got at, differences in the delicacy and complexity attainable, and of course differences in the slope of the curves for the same association. If all these tests agreed, we should have a right to rank one animal above the other in a scale of intelligence. But this whole question of grading is, after all, not so important for comparative psychology as its popularity would lead one to think. Comparative psychology wants first of all to trace human intellection back through the phylum to its origin, and in this aim is helped little by knowing that dogs are brighter than cats, or whales than seals, or horses than cows. Further, the whole question of 'intelligence' should be resolved into particular inquiries into the development of attention, activity, memory, etc.

So far as concerns dogs and cats, I should decide that the former were more generally intelligent. The main reason, however, why dogs seem to us so intelligent is not a good reason for the belief. It is because, more than any other domestic animal, they direct their attention to *us*, to what we do, and so form associations connected with acts of ours.

Having finished our attempt to give a true description of the facts of association, so far as observed from the outside, we may now progress to discuss its inner nature. A little preface about certain verbal usages is necessary before doing so. Throughout I shall use the word 'animal' or 'animals,' and the reader might fancy that I took it for granted that the associative processes were the same in all animals as in these cats and dogs of mine. Really, I claim for my animal psychology only that it is the psychology of just these particular animals. What this

warrants about animals in general may be left largely to the discretion of the reader. As I shall later say, it is probable that in regard to imitation and the power of forming associations from a lot of free ideas, the anthropoid primates are essentially different from the cats and dogs.

The reasons why I say 'animals' instead of 'dogs and cats of certain ages' are two. I do think that the probability that the other mammals, barring the primates, offer no objections to the theories here advanced about dogs and cats, is a very strong probability, strong enough to force the burden of proof upon any one who should, for instance, say that horse-goat psychology was not like cat-dog psychology in these general matters. I should claim that, till the contrary was shown in any case, my statements should stand for the mammalian mind in general, barring the primates. My second reason is that I hate to burden the reader with the disgusting rhetoric which would result if I had to insert particularizations and reservations at every step. The word 'animal' is too useful, rhetorically, to be sacrificed. Finally, inasmuch as most of my theorizing will be in the line of denying certain relatively high functions to animals, the evidence from cats and dogs is sufficient, for they are from among the most intelligent animals, and functions of the kind to be discussed, if absent in their case, are probably absent from the others.

Reasoning or Inference.

The first great question is whether or not animals are ever led to do any of their acts by reasoning. Do they ever conclude from inference that a certain act will produce a certain desired result, and so do it? The best opinion has been that they do not. The best interpretation of even the most extraordinary performances of animals has been that they were the result of accident and association or imitation. But it has after all been only opinion and interpretation, and the opposite theory persistently reappears in the literature of the subject. So, although it is in a way superfluous to give the *coup de grace* to the despised theory that animals reason, I think it is worth while to settle this question once for all.

The great support of those who do claim for animals the ability to infer has been their wonderful performances which resemble our own. These could not, they claim, have happened by accident. No animal could learn to open a latched gate by accident. The whole substance of the argument vanishes if, as a matter of fact, animals do learn those things by accident. *They certainly do.* In this investigation choice was made of the intelligent performances described by Romanes in the following passages. I shall quote at some length because these passages give an admirable illustration of an attitude of investigation which this research will, I hope, render impossible for any scientist in the future. Speaking of the general intelligence of cats, Romanes says:

"Thus, for instance, while I have only heard of one solitary case * * * of a dog which, without tuition, divined the use of a thumb-latch so as to open a closed door by jumping on the handle and depressing the thumb-piece, I have received some half-dozen instances of this display of intelligence on the part of cats. These instances are all such precise repetitions of one another that I conclude the fact to be one of tolerably ordinary occurrence among cats, while it is certainly very rare among dogs. I may add that my own coachman once had a cat which, certainly without tuition, learnt thus to open a door that led into the stables from a yard into which looked some of the windows of the house. Standing at these windows when the cat did not see me, I have many times witnessed her *modus operandi*. Walking up to the door with a most matter-of-course kind of air, she used to spring at the half hoop handle just below the thumb-latch. Holding on to the bottom of this half-hoop with one fore paw, she then raised the other to the thumb-piece, and while depressing the latter finally with her hind legs scratched and pushed the door-posts so as to open the door * * *.

"Of course in all such cases the cats must have previously observed that the doors are opened by persons placing their hands upon the handles and, having observed this, the animals act by what may be strictly termed rational imitation. But it should be observed that the process as a whole is something more than imitative. For not only would observation alone be scarcely enough (within any limits of thoughtful reflection that it would be reasonable to ascribe to an animal) to enable a cat upon the ground to distinguish that the essential part of the process consists not in grasping the handle, but in

depressing the latch; but the cat certainly never saw any one, after having depressed the latch, pushing the door-posts with his legs; and that this pushing action is due to an originally deliberate intention of opening the door, and not to having accidentally found this action to assist the process, is shown by one of the cases communicated to me; for in this case, my correspondent says, 'the door was not a loose-fitting one, by any means, and I was surprised that by the force of one hind leg she should have been able to push it open after unlatching it.' Hence we can only conclude that the cats in such cases have a very definite idea as to the mechanical properties of a door: they know that to make it open, even when unlatched, it requires to be *pushed*—a very different thing from trying to imitate any particular action which they may see to be performed for the same purpose by man. The whole psychological process, therefore, implied by the fact of a cat opening a door in this way is really most complex. First the animal must have observed that the door is opened by the hand grasping the handle and moving the latch. Next she must reason, by 'the logic of feelings'—'If a hand can do it, why not a paw?' Then strongly moved by this idea she makes the first trial. The steps which follow have not been observed, so we cannot certainly say whether she learns by a succession of trials that depression of the thumb-piece constitutes the essential part of the process, or, perhaps more probably, that her initial observations supplied her with the idea of clicking the thumb-piece. But, however this may be, it is certain that the pushing with the hind feet after depressing the latch must be due to adaptive reasoning unassisted by observation; and only by the concerted action of all her limbs in the performance of a highly complex and most unnatural movement is her final purpose attained." (Animal Intelligence, pp. 420–422.)

A page or two later we find a less ponderous account of a cat's success in turning aside a button and so opening a window:

"At Parara, the residence of Parker Bowman, Esq., a full-grown cat was one day accidentally locked up in a room without any other outlet than a small window, moving on hinges, and kept shut by means of a swivel. Not long afterwards the window was found open and the cat gone. This having happened several times, it was at last found that the cat jumped upon the window-sill, placed her fore-paws as high as she could reach against the side, deliberately reached with one over to the swivel, moved it from its horizontal to a vertical position, and then, leaning with her whole weight against

the window, swung it open and escaped." (Animal Intelligence, p. 425.)

A description has already been given on page 9 of the small box (C), whose door fell open when that button was turned, and also of a large box (CC) for the dogs,with a similar door. The thumb-latch experiment was carried on with the same box (G) for both cats and dogs, but the door was arranged so that a greater force (1.3 kilograms) was required in the case of the dogs. It will be remembered that the latch was so fixed that if the thumb-piece were pressed down, without contemporaneous outward pressure of the door, the latch bar would merely drop back into its catch as soon as the paw was taken off the door. If, however, the door were pushed outward the latch bar, being pressed closely against the outer edge of its catch, would, if lifted, be likely to fall outside it and so permit the door to open if then or later sufficient pressure were exerted. Eight cats (Nos. 1, 2, 3, 4, 5, 6, 7 and 13) were, one at a time, left in this thumb-latch box. All exhibited the customary instinctive clawings and squeezings and bitings. Out of the eight all succeeded in the course of their vigorous struggles in pressing down the thumb-piece, so that if the door had been free to swing open they could have escaped. Six succeeded in pushing both thumb-piece down and door out, so that the bar did not fall back into its place. Of these five succeeded in also later pushing the door open, so that they escaped and got the fish outside. Of these, three, after repeated trials, associated the complicated movements required with the sight of the interior of the box so firmly that they attacked the thumb-latch the moment they were put in. The history of the formation of the association in the case of 3 and of 4 is shown in the curves on page 21. In the case of 13 the exact times were not taken. The combination of accidents required was enough to make it take No. 1 and No. 6 a long time to get out. Consequently, weariness and failure inhibited their impulses to claw, climb, etc., more than the rare pleasure from getting out strengthened them, and they failed to form the association. Like the cats who utterly failed to get out, they finally ceased to try when put in. The history of their efforts is as follows: The figures in the columns represent

the time (in minutes and seconds) the animal was in the box before escaping or before being taken out if he failed to escape. Cases of failure are designated by an F after the figures. Double lines represent an interval of twenty-four hours.

No. 1.	No. 6.
13.00 F	17.50
9.30	3.30
1.40	9.00
.50	2.10
15.00	1.45
6.00 F	1.55
14.00	13.00
20.00 F	5.00
4.30	2.30
20.00 F	15.00
20.00 F	10.00 F
15.00 F	5.00
60.00 F	15.00 F
	10.00 F
	10.00 F

It should be noted that, although cats 3 and 4 had had some experience in getting out of boxes by clawing at loops and turning buttons, they had never had anything at all like a thumb-latch to claw at, nor had they ever seen the door opened by its use, nor did they even have any experience of the fact that the part of the box where the thumb-piece was was the door. And we may insert here, what will be stated more fully later, that there was displayed no observation of the surroundings or deliberation upon them. It was just a mad scramble to get out.

Three dogs (1, 2 and 3) were given a chance to liberate themselves from this same box. 2 and 3, who were rather inactive, failed to even push the thumb-piece down. No. 1, who was very active, did push it down at the same time that she hap-

pened to be pushing against the door. She repeated this and formed the association as shown in the curve on page 34. She had had experience only of escaping by pulling a loop of string.

Out of 6 cats who were put in the box whose door opened by a button, not one failed, in the course of its impulsive activity, to push the button around. Sometimes it was clawed one side from below; sometimes vigorous pressure on the top turned it around; sometimes it was pushed up by the nose. No cat who was given repeated trials failed to form a perfect association between the sight of the interior of that box and the proper movements. Some of these cats had been in other boxes where pulling a loop of string liberated them, 3 and 4 had had considerable experience with the boxes and probably had acquired a general tendency to claw at loose objects. 10, 11 and 12 had never been in *any box* before. The curves are on page 19.

Of two dogs, one, when placed in a similar but larger box, succeeded in hitting the button in such a way as to let the door open, and formed a permanent association, as shown by the curves on page 33. No one who had seen the behavior of these animals when trying to escape could doubt that their actions were directed by instinctive impulses, not by rational observation. It is then absolutely sure that a dog or cat *can* open a door closed by a thumb-latch or button, merely by the accidental success of its natural impulses. If *all* cats, when hungry and in a *small* box, will accidentally push the button that holds the door, an *occasional* cat in a *large* room may very well do the same. If three cats out of eight will accidentally press down a thumb-piece and push open a small door, three cats out of a thousand may very well open doors or gates in the same way.

But besides thus depriving of their value the facts which these theorizers offer as evidence, we may, by a careful examination of the method of formation of these associations as it is shown in the time-curves, gain positive evidence that no power of inference was present in the subjects of the experiments. Surely if 1 and 6 had possessed any power of inference they would not have failed to get out after having done so several times. Yet they did (see p. 43). If they had once even, much less if they

had six or eight times, inferred what was to be done, they should have made the inference the seventh or ninth time. And if there were in these animals any power of inference, however rudimentary, however sporadic, however dim, there should have appeared among the multitude some cases where an animal, seeing through the situation, knows the proper act, does it, and from then on does it immediately upon being confronted with the situation. There ought, that is, to be a sudden vertical descent in the time-curve. Of course, where the act resulting from the impulse is very simple, very obvious, and very clearly defined, a simple experience may make the association perfect, and we may have an abrupt descent in the time-curve without needing to suppose inference. But if in a complex act, a series of acts or an ill-defined act, one found such a sudden consummation in the associative process, one might very well claim that reason was at work. Now, the scores of cases recorded show no such phenomena. The cat does not look over the situation, much less *think* it over, and then decide what to do. It bursts out at once into the activities which instinct and experience have settled on as suitable reactions to the situation '*confinement when hungry with food outside*.' It does not ever in the course of its successes realize that such an act brings food and therefore decide to do it and thenceforth do it immediately from *decision* instead of from impulse. The one impulse, out of many accidental ones, which leads to pleasure, becomes strengthened and stamped in thereby, and more and more firmly associated with the sense-impression of that box's interior. Accordingly it is sooner and sooner fulfilled. Futile impulses are gradually stamped out. The gradual slope of the time-curve, then, shows the absence of reasoning. They represent the wearing smooth of a path in the brain, not the decisions of a rational consciousness.

In a later discussion of imitation further evidence that animals do not reason will appear. For the present, suffice it to say, that a dog, or cat, or chick, who does not in his own impulsive activity learn to escape from a box by pulling the proper loop, or stepping on a platform, or pecking at a door, will not learn it from seeing his fellows do so. They are incapable of

even the inference (if the process may be dignified by that name) that what gives another food will give it to them also. So, also, it will be later seen that an animal cannot learn an act by being put through it. For instance, a cat who fails to push down a thumb-piece and push out the door cannot be taught by having one take its paw and press the thumb-piece down with it. This *could* be learned by a certain type of associative process without inference. *Were there inference it surely would be learned.*

Finally, attention may be called to the curves which show the way that the animal mind deals with a series of acts (*e. g.*, curves for G, J, K, L and O, found on pages 21 to 26 and 34). Were there any reasoning the animals ought early to master the method of escape in these cases (see descriptions on pages 10 to 12 and 32) so as to do the several acts in order, and not to repeat one after doing it once, or else ought utterly to fail to master the thing. But, in all these experiments, where there was every motive for the use of any reasoning faculty, if such existed, where the animals literally lived by their intellectual powers, one finds no sign of abstraction, or inference, or judgment.

So far I have only given facts which are quite uninflenced by any possible incompetence or prejudice of the observer. These alone seem to disprove the existence of any rational faculty in the subjects experimented on. I may add that my observations of all the conduct of all these animals during the months spent with them, failed to find any act that even *seemed* due to reasoning. I should claim that this quarrel ought now to be dropped for good and all, that investigation ought to be directed along more sensible and profitable lines. I should claim that the psychologist who studies dogs and cats in order to defend this 'reason' theory is on a level with a zoölogist who should study fishes with a view to supporting the thesis that they possessed clawed digits. The rest of this account will deal with more promising problems, of which the first, and not the least important, concerns the facts and theories of *imitation*.

Imitation.

To the question, 'Do animals imitate?' science has uniformly answered, 'Yes.' But so long as the question is left in this general form, no correct answer to it is possible. It will be seen, from the results of numerous experiments soon to be described, that imitation of a certain sort is not possible for animals, and before entering upon that description it will be helpful to differentiate this matter of imitation into several varieties or aspects. The presence of some sorts of imitation does not imply that of other sorts.

There are, to begin with, the well-known phenomena presented by the imitative birds. The power is extended widely, ranging from the parrot who knows a hundred or more articulate sounds to the sparrow whom a patient shoemaker taught to get through a tune. Now, if a bird really gets a sound in his mind from hearing it and sets out forthwith to imitate it, as mocking-birds are said at times to do, it is a mystery and deserves closest study. If a bird, out of a lot of random noises that it makes, chooses those for repetition which are like sounds that he has heard, it is again a mystery *why*, though not as in the previous case a mystery *how*, he does it. The important fact for our purpose is that, though the imitation of sounds is so habitual, there does not appear to be any marked general imitative tendency in these birds. There is no proof that parrots do muscular acts from having seen other parrots do them. But this should be studied. At any rate, until we know what sort of sounds birds imitate, what circumstances or emotional attitudes these are connected with, how they learn them and, above all, whether there is in birds which repeat sounds any tendency to imitate in other lines, we cannot, it seems to me, connect these phenomena with anything found in the mammals or use them to advantage in a discussion of animal imitation as the forerunner of human. In what follows they will be left out of account, will be regarded as a specialization removed from the general course of mental development, just as the feathers or right aortic arch of birds are particular specializations of no consequence for the physical development of mammals. For

us, henceforth, imitation will mean imitation minus the phenomena of imitative birds.

There are also certain pseudo-imitative or semi-imitative phenomena which ought to be considered by themselves. For example, the rapid loss of the fear of railroad trains or telegraph wires among birds, the rapid acquisition of arboreal habits among Australian rodents, the use of proper feeding grounds, etc., may be held to be due to imitation. The young animal stays with or follows its mother from a specific instinct to keep near that particular object, to wit, its mother. It may thus learn to stay near trains, or scrabble up trees, or feed at certain places and on certain plants. Actions due to following pure and simple may thus simulate imitation. Other groups of acts which now seem truly imitative may be indirect fruits of some one instinct. This must be kept in mind when one estimates the supposed imitation of parents by young. Further, it is certain that in the case of the chick, where early animal life has been carefully observed, instinct and individual experience between them rob imitation of practically all its supposed influence. Chicks get along without a mother very well. Yet no mother takes more care of her children than the hen. Care in other cases, then, need not mean instruction through imitation.

These considerations may prevent an unreserved acceptance of the common view that young animals get a great number of their useful habits from imitation, but I do not expect or desire them to lead to its summary rejection. I should not now myself reject it, though I think it quite possible that more investigation and experiment may finally reduce all the phenomena of so-called imitation of parents by young to the level of indirect results of instinctive acts.

Another special department of imitation may be at least vaguely marked off: namely, apparent imitation of certain limited sorts of acts which are somewhat frequent in the animal's life. An example will do better than further definition.

Some sheep were being driven on board ship one at a time. In the course of their progress they had to jump over a hurdle. On this being removed before all had passed it, the next sheep was seen to jump as if to get over a hurdle, and so on for five

or six, apparently sure evidence that they imitated the action, each of the one in front. Now, it is again possible that among gregarious animals there may be elaborate connections in the nervous system which allow the sight of certain particular acts in another animal to arouse the innervation leading to those acts, but that these connections are *limited*. The reactions on this view are specific responses to definite signals, comparable to any other instinctive or associational reaction. The sheep jumps when he sees the other sheep jump, not because of a general ability to do what he sees done, but because he is furnished with the instinct to jump at such a sight, or because his experience of following the flock over boulders and brooks and walls has got him into the habit of jumping at the spot where he sees one ahead of him jump; and so he jumps even though no obstacle be in his way. If due to instinct the only peculiarity of such a reaction would be that the sense-impression calling forth the act would be the same act as done by another. If due to experience there would be an exact correspondence to the frequent acts called forth *originally* by several elements in a sense-impression, one of which is essential, and done *afterwards* when only the *non-essentials* are present. These two possibilities have not been sufficiently realized, yet they may contain the truth. On the other hand, these limited acts may be the primitive, sporadic beginnings of the general imitative faculty which we find in man. To this general faculty we may now turn, having cleared away some of the more doubtful phenomena which have shared its name.

It should be kept in mind that an imitative act may be performed quite unthinkingly, as when a man in the mob shouts what the others shout or claps when the others clap; may be done from an inference that since A by doing X makes pleasure for himself, I by doing X may get pleasure for myself; may, lastly, be done from what may be called a transferred association. This process is the one of interest in connection with our general topic, and most of my experiments on imitation were directed to the investigation of it. Its nature is simple. One sees the following sequence: 'A turning a faucet, A getting a drink.' If one can free this association from its narrow con-

finement to A, so as to get from it the association, 'impulse to turn faucet, *me* getting a drink,' one will surely, if thirsty, turn the faucet, though he had never done so before. If one can from an act witnessed learn to do the act, he in some way makes use of the sequence seen, transfers the process to himself; in the common human sense of the word, he *imitates*. This kind of imitation is surely common in human life. It may be apparent in ontogeny before any power of inference is shown. After that power does appear, it still retains a wide scope, and teaches us a majority, perhaps, of the ordinary accomplishments of our practical life.

Now, as the writers of books about animal intelligence have not differentiated this meaning from the other possible ones, it is impossible to say surely that they have uniformly credited it to animals, and it is profitless to catalogue here their vague statements. Many opposers of the 'reason' theory have presupposed such a process and used it to replace reason as the cause of some intelligent performances. The upholders of the reason theory have customarily recognized such a process and claimed to have discounted it in their explanations of the various anecdotes. So we found Mr. Romanes, in the passage quoted, discussing the possibility that such an imitative process, without reason, could account for the facts. In his chapter on Imitation in 'Habit and Instinct,' Principal C. Lloyd Morgan, the sanest writer on comparative psychology, seems to accept imitation of this sort as a fact, though he could, if attacked, explain most of his illustrations by the simple forms. The fact is, as was said before, that no one has analyzed or systematized the phenomena, and so one cannot find clear, decisive statements to quote.

At any rate, whether previous authorities have agreed that such a process is present or not, it is worth while to tackle the question; and the formation of associations by imitation, if it occurs, is an important division of the formation of associations in general. The experiments and their results may now be described.

Imitation in Chicks.

No. 64 learned to get out of a certain pen (16 × 10 inches) by crawling under the wire screening at a certain spot. There was also a chance to get out by walking up an inclined plane and then jumping down. No. 66 was put in with 64. After 9 minutes, 20 seconds, 66 went out by the inclined plane, although 64 had in the mean time crawled out under the screen 9 times. (As soon as he got out and ate a little he was put back.) It was impossible to judge how many of these times 66 really saw 64 do this. He was looking in that direction 5 of the times. So also, in three more trials, 66 used the inclined plane, though 64 crawled under each time. 67 was then tried. In 4 minutes, 10 seconds, he crawled under, 64 having done so twice. Being then put in *alone*, he, without the chance to imitate, still crawled under. So probably he went under *when with 64* not by imitation but by accident, just as 64 had learned the thing himself.

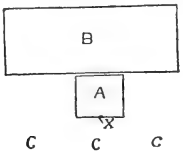

Fig. 17.

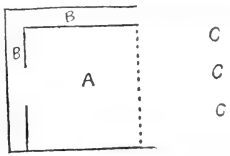

Fig. 18.

The accompanying figure (17) shows the apparatus used in the next experiment. A represents the top of a box (5 × 4 inches), 13 inches above the level of the floor, C. On the floor C were the chicks and food. B is the top of a box 10 inches high. Around the edges of A except the one next B a wire screen was placed, and 65 was repeatedly put upon A until he learned to go quickly back to C *via* B. Then the screen was bent outward at X so that a chick could barely squeeze through and down (A to C). Eleven chicks were then one at a time placed on A with 65. In every case but one they went A–C. In the case of the chick (75) who went A–B–C, there could have been no imitation, for he went down *before* 65 did. One other went through the hole before 65 went to B. The remain-

ing nine all had a chance to imitate 65 and to save the uncomfortable struggle to get through the hole, 65 going A–B–C 8 times before 68 went A–C, 2 times when with 66 and 76, once in the case of each of the others.

In still another experiment the apparatus was (as shown in figure 18) a pen 14 inches square, 10 inches high, with a wire screen in front and a hole 3½ inches square in the back. This hole opened into a passage-way (B) leading around to C, where were the other chicks and food. Chicks who had failed, when put in alone, to find the way out, were put in with other chicks who had learned the way, to see if by seeing them go out they would learn the way. Chick 70 was given 4 trials alone, being left in the box 76 minutes all told. He was then given 9 trials (165 minutes) with another chick who went out *via* B 36 times. 70 failed to follow him on any occasion. The trials were all given in the course of two days. Chick 73 failed in 1 trial (12 minutes) to get out of himself, and was then given 4 trials (94 minutes) with another chick who went out *via* B 33 times. In this experiment, as in all others reported, sure evidence that the animals wanted to get out, was afforded by their persistent peckings and jumpings at the screen or bars that stood between them and C. Chick 72, after 8 unsuccessful trials alone (41 minutes), was given 8 trials with a chance to imitate. After the other chick had gone out 44 times, 72 *did go out*. He did not follow the other but went 20 seconds later. It depends upon one's general opinion whether one shall attribute this one case out of three to accident or imitation.

I also took two chicks, one of whom learned to escape from A (in figure 17) by going to B and jumping down the side to the *right* of A, the other of whom learned to jump down the side to the *left*, and placed them together upon A. Each took his own course uninfluenced by the other in 10 trials.

Chicks were also tried in several pens where there was only one possible way of escape to see if they would learn it *more quickly* when another chick did the thing several times before their eyes. The method was to give some chicks their first trial with an imitation possibility and their second without, while others were given their first trial without and their second with.

If the ratio of the average time of the first trial to the average time of the second is smaller in the first class than it is in the second class, we may find evidence of this sort of influence by imitation. Though imitation may not be able to make an animal *do* what he would otherwise *not do*, it may make him do *quicker* a thing he would have done sooner or later any way. As a fact the ratio is *much larger*. This is due to the fact that a chick, when in a pen with another chick, is not afflicted by the discomfort of loneliness, and so does not try so hard to get out. So the other chick, who is continually being put in with him to teach him the way out, really prolongs his stay in. This factor destroys the value of these quantitative experiments, and I do not insist upon them as evidence against imitation, though they certainly offer none for it. I do not give descriptions of the apparatus used in these experiments or a detailed enumeration of the results, because in this discussion we are not dealing primarily with imitation as a slight general factor in forming experience, but as a definite associational process in the mind. The utter absence of imitation in this limited sense is apparently demonstrated by the results of the following experiments.

V was a box 16 × 12 × 8½, with the front made of wire screening and at the left end a little door held by a bolt but in such a way that a sharp peck at the top of the door would force it open.

W was a box of similar size, with a door in the same place fixed so that it was opened by raising a bolt. To this bolt was tied a string which went up over the top of the edge of the box and back across the box, as in D. By jumping up and coming down with the head over this thread, the bolt would be pulled up. The thread was 8½ inches above the floor.

X was a box of similar size, with door, bolt and string likewise. But here the string continued round a puiley at the back down to a platform in the corner of the box. By stepping on the platform the door was opened.

Y was a box 12 × 8 × 8½, with a door in the middle of the front, which I myself opened when a chick pecked at a tack which hung against the front of the box 1½ inches above the top of the door.

These different acts, pecking at a door, jumping up and with the neck pulling down a string, stepping on a platform, and pecking at a tack, were the ones which various chicks were given a chance to imitate. The chicks used were from 16 to 30 days old. The method of experiment was to put a chick in, leave him 60 to 80 seconds, then put in another who knew the act, and on his performing it, to let both escape. No cases were counted unless the imitator apparently saw the other do the thing. After about every ten such chances to learn the act, the imitator was left in alone for ten minutes. The following table gives the results. The imitators of course had previously failed to form the association of themselves. F denotes failure to perform the act:

Chick.	Act.	No. Times Saw.	Times in Which Failed.	Final Time.
84	V	38	45.00 F	15.00 F
85	V	30	30.00 F	10.00 F
86	V	44	55.00 F	15.00 F
87	V	26	35.00 F	15.00 F
80	W	54	60.00 F	15.00 F
81	W	40	45.00 F	15.00 F
87	W	27	30.00 F	10.00 F
81	X	18	20.00 F	10.00 F
82	X	21	20.00 F	8.40 *Did*
83	X	33	35.00 F	15.00 F
84	X	46	55.00 F	15.00 F
84	Y	45	55.00 F	15.00 F
83	Y	29	35.00 F	15.00 F

Thus out of all these cases only one did the act in spite of the ample chance for imitation. I have no hesitation in declaring 82's act in stepping on the platform the result of mere accident, and am sure that any one who had watched the experiments would agree.

Imitation in Cats.

By reference to the previous descriptions of apparatus, it will be seen that box D was arranged with two compartments, separated by a wire screen. The larger of these had a front of wooden bars with a door which fell open when a string stretched across the top was bitten or clawed down. The smaller was closed by boards on three sides and by the wire

screen on the fourth. Through the screen a cat within could see the one to be imitated pull the string, go out through the door thus opened and eat the fish outside. When put in this compartment, the top being covered by a large box, a cat soon gave up efforts to claw through the screen, quieted down and watched more or less the proceedings going on in the other compartment. Thus this apparatus could be used to test the power of imitation. A cat who had no experience with the means of escape from the large compartment was put in the closed one; another cat, who would do it readily, was allowed to go through the performance of pulling the string, going out, and eating the fish. Record was made of the number of times he did so and of the number of times the imitator had his eyes clearly fixed on him. These were called 'times seen.' Cases where the imitator was looking in the general direction of the 'imitatee' and might very well have seen him and probably did, were marked 'doubtful.' In the remaining cases the cat did not see what was done by his instructor. After the imitatee had done the thing a number of times, the other was put in the big compartment alone, and the time it took him before pulling the string was noted and his general behavior closely observed. If he failed in 5 or 10 or 15 minutes to do so, he was released and not fed. This entire experiment was repeated a number of times. From the times taken by the imitator to escape and from observation of the way that he did it, we can decide whether imitation played any part. The history of several cases are given in the following tables. In the first column are given the lengths of time that the imitator was shut up in the box watching the imitatee. In the second column is the number of times that the latter did the trick. In the third and fourth are the times that the imitator surely and possibly saw it done, while in the last is given the time that, when tried alone, the imitator took to pull the string, or if he failed, the time he was in the box trying to get out. Times are in minutes and seconds, failures denoted by F:

		No. 7 Imitating No. 2.			
	Time Watching.	No. of times 2 did.	No. of times 7 saw.	No. of times Doubtful.	Time of 7 when alone.
	10.00	11	3	5	
After 48 Hours	11.00	10	4	2	
	12.00	20	4	13	10.00 F
					1.00[1]
After 24 Hours	8.00	20	6	11	3.30
					10.00 F
	13.00	25	8	12	20.00 F
After 24 Hours	9.00	20	4	11	10.00 F
After 24 Hours	12.00	35	5	21	30.00 F
After 2 Hours	10.00	25	3	8	25.00 F
After 24 Hours	15.00	35	6	21	20.00 F
After 24 Hours	6.00	20	0	7	10.00 F
Total times surely and possibly seen,		-	43	111	

		No. 5 Imitating No. 2.			
	Time Watching.	No. of times 2 did.	No. of times 5 saw.	No. of times Doubtful.	Time of 5 when alone.
	12.00	15	3	8	5.00 F
After 2 Hours	10.00	8	4	4	
After 24 Hours	5.00	5	0	3	
After 1 Hour	14.00	10	5	3	10.00 F
After 1 Hour	13.00	22	7	11	10.00 F
After 24 Hours	7.00	15	3	8	5.00 F
After 48 Hours	18.00	20	2	9	20.00 F
After 24 Hours	14.00	20	2	10	30.00 F
After 24 Hours	10.00	20	7	12	20.00 F
Total times surely and possibly seen,		-	33	68	

		No. 6 Imitating No. 2.			
	Time Watching.	No. of times 2 did.	No. of times 6 saw.	No. of times Doubtful.	Time of 6 when alone.
	12.00	30	0	19	1.10[2]
After 48 Hours	11.00	30	0	11	9.30
After 72 Hours	10.00	30	0	15	3.00
After 72 Hours	6.00	20	3	7	1.50
After 24 Hours	9.00	30	1	13	10.00 F
After 24 Hours	10.00	30	6	9	10.00 F
After 24 Hours	10.00	30	1	8	9.40
Total times surely and possibly seen,		-	11	82	

[1] No. 7 hit the string in his general struggling, apparently utterly without design. He did not realize that the door was open till, two seconds after it had fallen, he happened to look that way.

[2] No. 6, in trying to crawl out at the top of the box, put its paw in above the string. It fell down and thus pulled the string. It did not claw *at* it, and it was 16 seconds before it noticed that the door was open. In all the other times that it escaped the movement was made in the course of promiscuous scrabbling, never in anything like the same way that No. 2 made it.

No. 3 Imitating No. 2.

	8.00	30	2	19	3.30[1]
					3.30
After 48 Hours	10.00	30	2	14	.20
					.20
After 72 Hours	10.00	30	2	8	.18
					.08
Total time surely and possibly seen,			6	41	

Before entering upon a discussion of the facts shown by these tables, we must describe the behavior of the imitators, when, after seeing 2 pull the string, they were put in alone. In the opinion of the present observer there was not the slightest difference between their behavior and that of cats 4, 10, 11, 12 and 13, who were put into the same position without ever having seen 2 escape from it. 6, 7, 5 and 3 paid no more attention to the string than they did, but struggled in just the same way. No one, I am sure, who had seen them, would have claimed that their conduct was at all influenced by what they had seen. When they did hit the string the act looked just like the accidental success of the ordinary association experiment. But, besides these personal observations, we have in the impersonal time-records sufficient proofs of the absence of imitation. If the animals pulled the string from having seen 2 do so, they ought to pull it in each individual case at an approximately regular length of time after they were put in, and presumably pretty soon thereafter. That is, if an association between the sight of that string in that total situation and a certain impulse and consequent freedom and food had been formed in their minds by the observation of the acts of 2, they ought to pull it *on seeing it*, and if any disturbing factor required that a certain time should elapse before the imitative faculty got in working order, that time ought to be somewhere near constant. The times were, as a fact, long and irregular in the extreme. Furthermore, if the successful cases were even in part due to imitation, the times ought to decrease the more they saw 2 do the thing. Except with 3, they *increase* or give place to failures. Whereas 6 and 7, if they had been put in again immediately after their first successful trial and

[1] No. 3 did not go out until 12 seconds had elapsed after it had pulled the string.

from then on repeatedly, would have unquestionably formed the association, they did not, when put in after a further chance to increase their knowledge by imitation, do the thing as soon as before. The case of 3 is not here comparable to the rest because he *was* given three trials in immediate succession. He was a more active cat and quicker to learn, as may be seen by comparing his time curves with those of 7, 6 and 5. That the mere speed with which he mastered this association is no sign that imitation was present may be seen by reference to the time curves of 4 and 13 (on page 18).

Some cats were also experimented with in the following manner. They were put into a box [No. 7 into box A (o at front), No. 5 into B (o at back)] and left for from 45 to 75 seconds. Then a cat who knew the way to get out was put in, and, of course, pulled at the loop and opened the door. *Both cats then went out and both were fed.* After the cat had been given a number of such chances to learn by imitation, he was put in and left until he did the thing, or until 5 or 10 minutes elapsed. As in the preceding experiments, no change in their behavior which might signify imitation was observed. No. 7 acted exactly like 3, or 10, or 11, when put in the box, apparently forming the association by accident in just the same way. Good evidence that he did not imitate is the fact that, whereas 1 (whom he saw) pulled the loop with his teeth, 7 pulled it with his paw. 5 failed to form the association, though he saw 3 do it 8 times and probably saw him 18 times more. He did get out twice by clawing the *string* in the *front* of the box, not the *loop* in the *back*, as 3 did. These successes took place early in the experiment. After that he failed when left alone to get out at all.

Another experiment was made by a still different method. My cats were kept in a large box about 4 ft. high, the front of which was covered with poultry-yard netting. Its top was a board which could be removed. To save opening the door and letting them all loose, I was in the habit of taking them out by the top when I wanted to experiment with them. Of course the one who happened to climb up (perhaps attracted by the smell of fish on my fingers) was most likely to be taken out and experi-

mented with and fed. Thus they formed the habit of climbing up the front of the box whenever I approached. Of three cats which I obtained at the same time, one did not after 8 or 10 days acquire this habit. Even though I held out a piece of fish through the netting, he would not climb after it. It was reasonable to suppose that imitation might overcome this sluggishness, if there were any imitation. I therefore put two cats with him and had them climb up 80 times before his eyes and get fish. He never followed or tried to follow them.

4 and 3 had been subjected to the following experiment. I would make a certain sound and after 10 seconds would go up to the cage and hold the fish out to them through the netting at the top. They would then, of course, climb up and eat it. After a while, they began to climb up upon hearing the signal (4) or before the 10 seconds were up. I then took 12 and 10, who were accustomed to going up when they saw me approach, but who had no knowledge of the fact that the signal meant anything, and gave them each a chance to imitate 3. That is one of them would be left in the box with 3, the signal would be given, and after from 5 to 10 seconds 3 would climb up. At 10 seconds I would come up with food, and then of course 12 would climb up. This was repeated again and again. The question was whether imitation would lead them to form the association more quickly than they would have done alone. It did not. That when at last they did climb up before 10 seconds was past, that is before I approached with food, it was not due to imitation, is shown by the fact that on about half of such occasions they climbed up *before 3 did*. That is they reacted to the *signal* by *association*, not to his *movements* by *imitation*.

Imitation in Dogs.

Here the method was not to see if imitation could arouse more quickly an act which accident was fairly likely to bring forth sooner or later, but to see if, where accident failed, imitation would succeed.

3 was found to be unable of himself to escape from box BB1, and was then given a chance to learn from watching 1. The

back of box BB1 was torn off and wire netting substituted for it. Another box with open front was placed directly behind and against box BB1. No. 3, who was put in this second box, could thus see whatever took place in and in front of box BB1 (o at back, high). The record follows:

		Dog 3 Imitating Dog 1.		
	Times 1 did.	Times 3 saw.	Times probably saw.	Times in alone.
	30	7	14	3.00 F
After 1 Hour - -	35	9	14	3.00 F
After 1 Hour - -	10	3	3	5.00 F
After 24 Hours - -	20	6	8	
	30	8	13	6.00 F
After 48 Hours - -	25	8	11	8.00 F
	25	6	12	6.00 F
	25	9	7	10.00 F
After 24 Hours - -	30	10	11	40.00 F
Total times surely and possibly seen, -		66	93	

A similar failure to imitate was observed in the case of another simple act. No. 1, as may be seen on page 33, had learned to escape from a pen about 8 by 5 feet by jumping up and biting a cord which ran from one end of the pen to the other and at the front end was tied to the bolt which held the door. Dogs 2 and 3 had failed in their accidental jumping and pawing to hit this cord, and were then given a chance to learn by seeing 1 do so, escape, and of course be fed. 1 always jumped in the same way, biting the cord at the same place, namely where a loose end from a knot in it hung down 4 or 5 inches. 2 and 3 would either be tied up in the pen or left in a pen at one side. They had a perfect chance to see 1 perform his successful act. After every twenty or thirty performances by 1, 2 and 3 would be put in alone. It should be remembered that here, as also in the previous experiment and all others, the imitators certainly *wanted* to get out when thus left in alone. They struggled and jumped and pawed and bit, but they never jumped *at the cord.* Their records follow.

Dog 2 Imitating Dog 1.

	Times 1 did.	Times 2 saw.	Times Doubtful.	Time 2 was in alone
	30	9	11	10.00 F
After 1 Hour, - -	30	10	9	10.00 F
After 48 Hours, - -	25	8	8	
After 1 Hour, - -	10	3	4	9.00 F [1]
After 24 Hours, - -	30	8	12	15.00 F
After 1 Hour, - -	30	9	12	15.00 F
After 48 Hours, - -	20	7	6	10.00 F
	20	8	7	
After 48 Hours, - -	30	6	8	15.00 F
After 24 Hours, - -	15	2	4	10.00 F
Total times surely and possibly seen, -	70		81	

Dog 3 Imitating Dog 1.

	Time 1 did.	Times 3 saw.	Times Doubtful.	Time 3 was in alone.
	30	10	10	10.00 F
After 1 Hour, - -	30	9	10	10.00 F
After 1 Hour, - -	15	6	4	
After 24 Hours, - -	30	9	11	15.00 F
After 24 Hours, - -	30	10	12	15.00 F
After 1 Hour, - -	30	8	9	10.00 F
After 48 Hours, - -	20	6	7	40.00 F
After 1 Hour, - -	20	6	5	
After 48 Hours, - -	30	8	9	15.00 F
After 24 Hours, - -	15	3	4	20.00 F
Total times surely and possibly seen, -	75		81	

Another corroborative, though not very valuable, experiment was the following: Dog 3 had been taught for the purpose of another experiment to jump up on a box and beg when I held a piece of meat above the box. I then caused him to do this 110 times (within two days) in the presence of 1. Although 1 saw him at least 20 % of the times (3 was always fed each time he jumped on the box), he never tried to imitate him.

It seems sure from these experiments that the animals were unable to form an association leading to an act from having seen the other animal, or animals, perform the act in a certain

[1] The back of the pen adjoined the elevator shaft, being separated from it by a partition 33 inches high. No. 2 heard the elevator coming up and put his paws up on the top of this partition so as to look over. In so doing he knocked the fastening of the cord at that end and opened the door. He did not turn to come out, and I shut the door again.

situation. Thus we have further restricted the association process. Not only do animals not have associations accompanied, more or less permeated and altered, by inference and judgment; they do not have associations of the sort which may be acquired from other animals by imitation. What this implies concerning the actual mental content accompanying their acts will be seen later on. It also seems sure that we should give up imitation as an *a priori* explanation of any novel intelligent performance. To say that a dog who opens a gate, for instance, need not have reasoned it out *if he had seen another dog do the same thing*, is to offer instead of one false explanation another equally false. Imitation in any form is too doubtful a factor to be presupposed without evidence. And if a general imitative faculty is not sufficiently developed to succeed with such simple acts as those of the experiments quoted, it must be confessed that the faculty is in these higher mammals still rudimentary and capable of influencing to only the most simple and habitual acts, or else that for some reason its sphere of influence is limited to a certain class of acts, possessed of some *qualitative difference*, other than mere simplicity, which renders them imitable. The latter view seems a hard one to reconcile with a sound psychology of imitation or association at present, without resorting to instinct. Unless a certain class of acts are by the innate mental make-up especially tender to the influence of imitation, the theory fails to find good psychological ground to stand on. The former view may very well be true. But in any case the burden of proof would now seem to rest upon the adherents to imitation; the promising attitude would seem to be one which went without imitation as long as it could, and that is, of course, until it surely found it present.

Returning to imitation considered in its human aspect, to imitation as a transferred association in particular, we find that here our analytical study of the animal mind promises important contributions to general comparative psychology. If it is true, and there has been no disagreement about it, that the primates do imitate acts of such novelty and complexity that only this out-and-out kind of imitation can explain the fact, we have located one great advance in mental development. Till the primates

we get practically nothing but instincts and individual acquirement through impulsive trial and error. Among the primates we get also acquisition by imitation, one form of the increase of mental equipment by tradition. The child may learn from the parent quickly without the tiresome process of seeing for himself. The less active and less curious may share the progress of their superiors. The brain whose impulses hitherto could only be dislodged by specific sense-impressions may now have any impulse set agoing by the sight of the movement to which it corresponds.

All this on the common supposition that the primates *do* imitate, that a monkey in the place of these cats and dogs *would* have pulled the string. My apology for leaving the matter in this way without experiments of my own is that the monkey which I procured for just this purpose failed in two months to become tame enough to be thus experimented on. Accurate information about the nature and extent of imitation among the primates should be the first aim of further work in comparative psychology, and will be sought by the present writer as soon as he can get subjects fit for experiments.

In a questionnaire which was sent to fifteen animal-trainers, the following questions were asked:

1. "If one dog was in the habit of 'begging' to get food and another dog saw him do it ten or twenty times, would the second dog then beg himself?"

2. "In general is it easier for you to teach a cat or dog a trick if he has seen another do it?"

3. "In general do cats imitate each other? Do dogs? Do monkeys?"

4. "Give reasons for your opinion, and please write all the reasons you have."

Five gentlemen (Messrs. R. C. Carlisle, C. L. Edwards, V. P. Wormwood, H. S. Maguire and W. E. Burke) courteously responded to my questionnaire. All are trainers of acknowledged reputation. To these questions on imitation four replied.

To the first question we find the following answers: (*a*) "Most dogs would." (*b*) "Yes; he will very likely do it. He will try and imitate the other dog *generally*." (*c*) "If a young dog with the mother, it would be very apt to . . . With older dogs, it would depend very much upon circumstances." (*d*) "He would not."

To 2 the answers were: (*a*) "Very much easier." (*b*) "It is always easier if they see another one do it often." (*c*) "This would also depend on certain conditions. In teaching to jump out of a box and in again, seeing another might help, but in teaching something very difficult, I do not think it would be the case." (*d*) "It is not."

To 3 the answers were: (*a*) "Yes. Some. More than either dogs or cats." (*b*) "Yes. Yes. Yes." (*c*) "In certain things, yes; mostly in those things which are in compliance to the laws of their own nature." (*d*) "No. No. Yes, they are born imitators."

The only definite answer to question 4 was: "Take a dog or cat and close them up in a room and go in and out several times, and you will find that they will go to the door and stand up on their hind legs with front paws on the door knob and try to open the door to get out. I could also give you a hundred more such reasons." This was given by (*b*).

The replies to a test question, however, go to show that these opinions regarding imitation may be mistaken. Question 8 was: "If you wanted to teach a cat to get out of a cage by opening an ordinary thumb-latch and then pushing the door, would you take the cat's paw and push down the thumb-piece with it and then push the door open with the paw, or would you just leave the cat inside until it learned the trick itself?" The second is certainly the better way, as will be seen in a later part of this paper, and pushing the latch with the cat's paw has absolutely no beneficial influence on the formation of the association, yet (*a*) and (*b*) both chose the first way, and (*c*) answered ambiguously. Further, the only reason given is, of course, no reason at all. It proves too much, for if there were such imitation as that, my cats and dogs would surely have done the far simpler things required of them. I cannot find that trainers make any practical use of imitation in teaching animals tricks, and on the whole I think these replies leave the matter just where it was before. They are mere opinions—not records of observed facts. It seems arrogant and may seem to some unjustifiable thus to discard testimony, to stick to a theory based on one's own experiments in the face of these opinions. If I had wished to gain applause and avoid adverse criticism, I would have abstained from upholding the radical view of the preceding pages. At times it seems incredible to me that the results of my experiments should embody the truth of the matter, that there should be no imitation. The theory based on them seems, even to me, too radical, too novel. It seems highly improbable that I should be right and all the others wrong. But I cannot avoid the responsibility of giving what seems to my judgment the most probable explanation of the results of the experiments; and that is the radical explanation already given.

The Mental Fact in Association.

It is now time to put the question as to just what is in an animal's mind when, having profited by numerous experiences, he has formed the association and does the proper act when put in a certain box. The commonly accepted view of the mental fact then present is that the sight of the inside of the box reminds the animal of his *previous pleasant experience after escape* and *of the movements* which he made which were immediately followed by and so associated with that escape. It has been taken for granted that *if the animal remembered the pleasant experience and remembered the movement, he would make the movement.* It has been assumed that the association was *an association of ideas;* that when one of the ideas was of a movement the animal was capable of making the movement. So, for example, Morgan says, in the 'Introduction to Comparative Psychology': "If a chick takes a lady-bird in its beak forty times and each time finds it nasty, this is of no practical value to the bird unless the sight of the bird suggests *the nasty taste*," p. 90.

Again, on page 92, Morgan says, "*A race after the ball* had been suggested through the channel of olfactory sensations." Also on page 86 "* * * the visual impression suggested the idea or representation of unpleasant gustatory experience." The attitude is brought out more completely in a longer passage on page 118: "On one of our first ascents one of them put up a young coney, and they both gave chase. Subsequently they always hurried on to this spot, and, though they never saw another coney there, reiterated disappointment did not efface *the memory of that first chase*, or so it seemed." That is, according to Morgan, the dogs thought of the chase and its pleasure, on nearing the spot where it had occurred, and so hurried on. On page 148 of 'Habit and Instinct,' we read, "Ducklings so thoroughly associated water with the sight of their tin that they tried to drink from it and wash in it when it was empty, nor did they desist for some minutes," and this with other similar phenomena is attributed to the 'association by contiguity' of human psychology.

From these quotations it seems fairly sure that if we should ask Mr. Morgan, who is our best comparative psychologist, what took place in the mind of one of these cats of our experiments during the performance of one of the 'tricks,' he would reply: "The cat performs the act because of the association of ideas. He is reminded by the sight of the box and loop of his experience of pulling that loop and of eating fish outside. So he goes and pulls it again." This view has stood unchallenged, but its implication is false. It implies that an animal, whenever it thinks of an act, can supply an *impulse to do* the act. It takes for granted that the performance of a cat who gets out of a box is mentally like that of a man who thinks of going down street or of writing a letter and then does it. The mental process is not alike in the two cases, for animals can *not* provide the impulse to *do* whatever act they think *of*. *No cat can form an association leading to an act unless there is included in the association an impulse of its own which leads to the act.* There is no general storehouse from which the impulse may be supplied after the association is formed.

Before describing the experiments which justify these statements, it will be worth while to recall the somewhat obvious facts about the composition of one of these associations. There might be in an association, such as is formed after experience with one of our boxes, the following elements:

1. Sense-impression of the interior of the box, etc.
2. (*a*) Discomfort and (*b*) desire to get out.
3. Representation of oneself pulling the loop.
4. Fiat comparable to the human "I'll do it."
5. The impulse which actually does it.
6. Sense-impression of oneself pulling the loop, seeing one's paw in a certain place, feeling one's body in a certain way, etc.
7. Sense impression of going outside.
8. Sense-impression of eating, and the included pleasure.

Also between 1 and 4 we may have 9, representations of one's experience in going out, 10, of the taste of the food, etc.

6, 7 and 8 come after the act and do not influence it, of course, except in so far as they are the basis of the future 3's, 9's and 10's. About 2 we are not at present disputing. Our

question is as to whether 3 or 5 is the essential thing. In human associations 3 certainly often is, and the animals have been credited with the same kind. Whatever he *thinks*, Professor Morgan surely *talks* as if 1 aroused 9 and 10 and 3 and leaves 5 to be supplied at will. We have affirmed that 5 is the essential thing, that no association without a specific 5 belonging to it and acquired by it can lead to an act. Let us look at the reasons.

A cat has been made to go into a box through the door, which is then closed. She pulls a loop and comes out and gets fish. She is made to go in by the door again, and again lets herself out. After this has happened enough times, the cat will of her own accord go into the box after eating the fish. It will be hard to keep her out. The old explanation of this would be that the cat associated the memory of being in the box with the subsequent pleasure, and therefore performed the equivalent of saying to herself, "Go to! I will go in." The thought of *being in*, they say, makes her *go in*. *The thought of being in will not make her go in*. For if, instead of pushing the cat toward the doorway or holding it there, and thus allowing it to itself give the impulse, to innervate the muscles, to walk in, you shut the door first and drop the cat in through a hole in the top of the box, she will, after escaping as many times as in the previous case, *not* go into the box of her own accord. She has had exactly the same opportunity of connecting the idea of being in the box with the subsequent pleasure. Either a cat cannot connect ideas, representations, at all, or she has not the power of progressing from the thought of being in to the act of going in. The only difference between the first cat and the second cat is that the first cat, in the course of the experience, has the impulse to crawl through that door, while the second has not the impulse to crawl through the door or to drop through that hole. So, though you put the second cat on the box beside the hole, she doesn't try to get into the box through it. The impulse is the *sine qua non* of the association. The second cat has everything else, but cannot supply that. These phenomena were observed in six cats, three of which were tried by the first method, three by the second. Of

the first three, one went in himself on the 26th time and frequently thereafter, one on the 18th and the other on the 37th; the two last as well as the first did that frequently in later trials. The other three all failed to go in themselves after 50, 60 and 75 trials, respectively.

The case of No. 7 was especially instructive, though not among these six. No. 7 had had some trials in which it was put in through the door, but ordinarily in this particular experiment was dropped in. After about 80 trials it would frequently exhibit the following phenomena: It would, after eating the fish, go up to the doorway and, rushing from it, search for fish. The kitten was very small and would go up into the doorway, whirl round and dash out, all in one quick movement. The best description of its behavior is the paradoxical one that it went out without going in. The association evidently concerned what it had *done*, what it had an impulse for, namely, *coming out through that door* to get fish, not what it remembered, had a representation of.

Still more noteworthy evidence is found in the behavior of cats and dogs who were put in these boxes, left one or two minutes, and then put through the proper movement. For example, a cat would be put in B (o at back) and left two minutes. I would then put my hand in through the top of the box, take the cat's paw and with it pull down the loop. The cat would then go out and eat the fish. This would be done over and over again, and after every ten or fifteen such trials the cat would be left in alone. If in ten or twenty minutes he did not escape, he would be taken out through the top and not fed. In one series of experiments animals were taken and thus treated in boxes from which their own impulsive activity had failed to liberate them. The results, given in the table below, show that no animal who fails to perform an act in the course of his own impulsive activity will learn it by being put through it.

In these experiments some of the cats and all of the dogs but No. 1 showed no agitation or displeasure at my handling from the very start. Nor was there any in Dog 1 or the other cats after a few trials. It may also be remarked that in the trials alone which took place during and at the end of the ex-

periment the animals without exception showed that they did not fail to perform the act from lack of a desire to get out. They all tried hard enough to get out and would surely have used the association if they had formed it.

Individual.	Apparatus.	Time in which impulsive activity failed to lead to the act.	Number of times the animal was put through the movement.	Time in which this experience failed to lead to the act.	Time of final trial
Cat 1	F (String outside unfastened)	55.00	77	120.00	20.00
Cat 5	G (Thumb-latch) - -	57.00	59	55.00	10.00
Cat 7	G (Thumb-latch) - -	50.00	30	35.00	10.00
Cat 2	G (Thumb-latch) - -	54.00	141	110.00	20.00
Dog 2	BB1 (o at back, high) -	48.00	30	80.00	10.00
Dog 3	BB1 (o at back, high) -	20.00	85	55.00	10.00
Dog 2	M (Lever outside) - -	15.00	95	140.00	30.00
Dog 1	FF[1] - - - - -	30.00	110	135.00	60.00
Chick 89	X (See page 53).	20.00	30	60.00	30.00
Cat 13	KKK[2, 3] - - - -	40.00	65	60.00	10.00

Now, the only difference between the experiences of the animals in these experiments and their experiences in those where they let themselves out, is that here they only saw and felt themselves making the movement, whereas in the other case they also felt the impulse, gave the innervation. That, then, is the essential. It may be objected that the animals failed because they did not *attend* to the process of being put through the movement, that, had they attended to it, they would later themselves have made the movement. It is, however, improbable that out of fifty times an animal should not have attended to what was going on at least two or three times. But if seeing himself do it was on a par with feeling an impulse to and so doing it, even two or three times would suffice to start the habit. And it is even more im-

[1] FF was a box 40 × 21 × 24 inches, the door of which could be opened by putting the paw out between the bars to its right and pulling a loop which hung 16 inches above the floor, 4 inches out from the box and six inches to the right of the door.

[2] KKK was box K with both bolts removed. All that had to be done was to poke the paw out at one side of the door and press down a little bar of wood.

[3] The cats and chick were left in for two minutes at each trial, the dogs for from one to one and a half minutes.

probable that an experience should be followed by keen pleasure fifty times and not be attended to with might and main, unless animals attend *only* to their own impulses and the excitements thereof. But if the latter be true, it simply affirms our view from a more fundamental standpoint.

In another set of experiments animals were put in boxes with whose mechanism they had had no experience, and from which they might or might not be able to escape by their own impulsive acts. The object was to see whether the time taken to form the association could be altered by my instruction. The results turned out to give a better proof of the inability to form an association by being put through the act than any failure to change the time-curve. For it happened in all but one of the cases that the movement which the animal made to open the door was different from the movement which I had put him through. Thus, several cats were put through (in Box C [Button]) the following movement: I took the right paw and, putting it against the lower right-hand side of the button, pushed it round to a horizontal position. The cats' ways were as follows: No. 1 turned it by clawing vigorously at its top; No. 6, by pushing it round with his nose; No. 7, in the course of an indiscriminate scrabble at first, in later trials either by pushing with his nose or clawing at the top, settling down finally to the last method. Nos. 2 and 5 did it as No. 1 did. Cat 2 was tried in B (0 at back). I took his paw and pressed the loop with it, but he formed the habit of clawing and biting the string at the top of the box near the front. No. 1 was tried in A. I pressed the loop with his paw, but he formed the habit of biting at it.

In every case I kept on putting the animal through the act every time, if at the end of two minutes (one in several cases) it had not done it, even after it had shown, by using a different way, that my instruction had no influence. I never succeeded in getting the animal to change its way for mine. Moreover, if any one should fancy that the animal really profited by my instruction so as to learn what result to attain, namely, the turning of a certain button, but chose a way of his own to turn it, he would be deluding himself. The time taken to learn the act with instruction was no shorter than without.

If, then, an animal happens to learn an act by being put through it, it is just happening, nothing more. Of course, you may *direct* the animal's efforts so that he will perform the act himself the sooner. For instance, you may hold him so that his accidental pawing will be sure to hit the vital point of the contrivance. But the animal can not form an association leading to an act unless the particular impulse to that act is present as an element of the association; he cannot supply it from a general stock. The groundwork of animal associations is not the association of *ideas*, but the association of idea or sense-impression with *impulse*.

In the questionnaire mentioned elsewhere, some questions were asked with a view to obtaining corroboration or refutation of this theory that an impulse or innervation is a necessary element in every association formed if that association leads to an act. The questions and answers were:

Question 1: "If you wanted to teach a horse to tap seven times with his hoof when you asked him, "How many days are there in a week?," would you teach him by taking his leg and making him go through the motions?"

A answered, "Yes! at first."

B answered, "No! I would not."

C answered, "At first, yes!"

D answered, "No!"

Question 2: "Do you think you *could* teach him that way, even if naturally you would take some other way?"

A answered, "In time, yes!"

B answered, "I think it would be a very hard way."

C answered, "Certainly I do."

D answered, "I do not think I could."

E answered, "Yes."

Question 3: "How would you teach him?"

A answered, "I should tap his foot with a whip, so that he would raise it, and reward him each time."

B answered, "I should teach him by the motion of the whip."

C answered, "First teach him by pricking his leg the number of times you wanted his foot lifted."

D answered, "You put figure 2 on blackboard and touch him on leg twice with cane, and so on."

E answered ambiguously.

It is noteworthy that even those who think they *could* teach an animal by putting him through the trick do not use that method, except at first. And what they really do then is probably to stimulate the animal to the reflex act of raising his hoof. The hand simply replaces the cane or whip as the means of stimulus. The answers are especially instructive, because the numerous counting tricks done by trained horses seem, at first, to be incomprehensible, unless the trainer can teach the horse by putting it through the movement the proper number of times. The counting tricks performed by Mascot, Professor Maguire's horse, were quoted to me by a friend as incomprehensible on my theory. The answers given above show how simple the thing really is. All the counting-tricks of all the intelligent horses depend on the fact that a horse raises his hoof when a certain stimulus is given. One simple reaction gives the basis for a multitude of tricks. In the same way other tricks, which at first sight seem to require that the animal should learn by being put through the movement, may depend on some simple reflex or natural impulse.

Another question was, "How would you teach a cat to get out of a box, the door of which was closed with a thumb-latch?"

A answered, "I should use a puff-ball as a plaything for the cat to claw at." This means, I suppose, that he would get the cat to claw at the puff-ball and thus direct its clawings to the vicinity of the thumb-piece.

B answered, "I would put the cat in and get it good and hungry and then open the door by lifting the latch with my finger. Then put some food that the cat likes outside, and she will soon try to imitate you and so learn the trick."

C answered, "I would first adjust all things in connection with the surroundings of the cat so they would be applicable to the laws of its nature, and then proceed to teach the trick."

I suppose this last means that he would fix the box so that some of the cat's instinctive acts would lead it to perform the trick. The answer given by *B* means apparently that he would

simply leave the thing to accident, for any such imitation as he supposes is out of the question. At all events, none of these would naturally start to teach the trick by putting the animal through the motions, which, were it a possible way, would probably be a traditional one among trainers. On the whole, I see in these data no reason for modifying our dogma that animals cannot learn acts without the impulse.

Presumably the reader has already seen budding out of this dogma a new possibility, a further simplification of our theories about animal consciousness. The possibility is that animals may have *no images or memories at all, no ideas to associate.* Perhaps the entire fact of association in animals is the presence of sense-impressions with which are associated, by resultant pleasure, certain impulses, and that therefore, and therefore only, a certain situation brings forth a certain act. Returning to our analysis of the association, this theory would say that there was no (9) or (10) or (3) or (4), that the sense-impression gave rise, when accompanied by the feeling of discomfort, to the impulse (5) directly, without the intervention of any representations of the taste of the food, or the experience of being outside, or the sight of oneself doing the act. This theory might be modified so as to allow that the representations could be there, but to deny that they were necessary, were inevitably present, that the impulse was connected to the sense-impression through them. It would then claim that the effective part of the association was a direct bond between the situation and the impulse, but would not cut off the possibility of there being an aura of memories along with the process. It then becomes a minor question of interpretation which will doubtless sooner or later demand an answer. I shall not try to answer it now. The more radical question, the question of the utter exclusion of representative trains of thought, of any genuine association of *ideas* from the mental life of animals, is worth serious consideration. I confess that, although certain authentic anecdotes and certain experiments to be described soon, lead me to reject this exclusion, there are many qualities in animals' behavior which seem to back it up. If one takes his stand by a rigid application of the law of parsimony, he will find justification for this view which no experiments of mine can overthrow.

Of one thing I am sure, and that is that it is worth while to state the question and how to solve it, for although the point of view involved is far removed from that of our leading psychologists to-day, it cannot long remain so. I am sorry that I cannot pretend to give a final decision.

The view seems preposterous because, if an animal has sense-impressions when his brain is excited by currents starting in the end-organs, it seems incredible that he should not be conscious in imagination and memory by having similar excitations caused from within. We are accustomed to think of memory as the companion of sensation. But, after all, it is a question of fact whether the connections in the cat-brain include connections between present sensation-neuroses and past sensation-neuroses. The only connections may be those between the former and impulse-neuroses, and there is no authoritative reason why we should suppose any others unless they are demonstrated by the cat's behavior. This is just the point at issue. Such evidence as the phenomena of animals' dreams does not at all prove the presence of memory or imagination. A dog may very well growl in his sleep without any idea of a hostile dog. The impulse to growl *may* be caused by chance excitement of its own neurosis without any sensation-neurosis being concerned. *Acts* of recognition may have no *feelings* of recognition going with or causing them. A sense-impression of me gets associated in my dog's mind with the impulses to jump on me, lick my hand, wag his tail, etc. If, after a year, the connection between the two has lasted, he will surely jump on me, lick my hand and wag his tail, though he has not and never had any representation of me.

The only logical way to go at this question and settle it is, I think, to find some association the formation of which requires the presence of images, of ideas. You have to give an animal a chance to associate sense-impression A with sense-impression B and then to associate B with some act C so that the presence of B in the mind will lead to the performance of C. Presumably the representation of B, if present, will lead to C just as the sense-impression B did. Now, if the chance to associate B with A has been improved, you ought, when the

animal is confronted with the sense-impression A, to get a revival of B and so the act C. Such a result would, if all chance to associate C with A had been eliminated, demonstrate the presence of representations and their associations. I performed such an experiment in a form modified so as to make it practicable with my animals and resources. Unfortunately, this modification spoils the crucial nature of the experiment and robs it of much of its authority. The experiment was as follows:

A cat was in the big box where they were kept (see page 58) very hungry. As I had been for a long time the source of all food, the cats had grown to watch me very carefully. I sat, during the experiment, about eight feet from the box, and would at intervals of two minutes clap my hands four times and say, "I must feed those cats." Of course the cat would at first feel no impulse except perhaps to watch me more closely when this signal was given. After ten seconds had elapsed I would take a piece of fish, go up to the cage and hold it through the wire netting, three feet from the floor. The cat would then, of course, feel the impulse to climb up the front of the cage. In fact, experience had previously established the habit of climbing up whenever I moved toward the cage, so that in the experiment the cat did not ordinarily wait until I arrived there with the fish. In this experiment:

A=The sense-impression of my movements and voice when giving the signal.

B=The sense-impression of my movements in taking fish, rising, walking to box, etc.

C=The act of climbing up, with the impulse leading thereunto.

The question was whether after a while A would remind the cat of B, and cause him to do C before he got the *sense-impression* of B, that is, before the ten seconds were up. If A leads to C through a memory of B, animals surely *can* have association of ideas proper, and probably often *do*. Now, as a fact, after from thirty to sixty trials, the cat does perform C immediately on being confronted by A or some seconds later, at all events before B is presented. And it is my present opinion

that their action is to be explained by the presence, through association, of the idea B. But it is not impossible that A was associated *directly* with the impulse to C, although that impulse was removed from it by ten seconds of time. Such an association is, it seems to me, highly improbable, unless the neurosis of A, and with it the psychosis, continues until the impulse to C appears. But if it does so continue during the ten seconds, and thus get directly linked to C, we have exactly a representation, an image, a memory, in the mind for eight of those ten seconds. It does not help the deniers of images to substitute an image of A for an image of B. Yet, unless they do this they have to suppose that A comes and goes, and that after ten seconds C comes, and, passing over the intervening blank, willfully choses out A and associates itself with it. There are some other considerations regarding the behavior of the cats from the time the signal was given till they climbed up, which may be omitted in the hope that it will soon be possible to perform a decisive experiment. If an observer can make sure of the animal's attention to a sequence A——B, where B does not arouse any impulse to an act, and then later get the animal to associate B with C, leaving A out this time, he may then, if A, when presented anew, arouses C, bid the deniers of representations to forever hold their peace.

Another reason for allowing animals representations and images is found in the longer time taken to form the association between the act of licking or scratching and the consequent escape. If the associations in general were simply between situation and impulse and act, one would suppose that the situation would be associated with the impulse to lick or scratch as readily as with the impulse to turn a button or claw a string. Such is not the case. By comparing the curves for Z on pages 22, 23 with the others one sees that for so simple an act it takes a long time to form the association. This is not a final reason, for lack of attention, a slight increase in the time taken to open the door after the act was done, or an absence of preparation in the nervous system for connections between these particular acts and definite sense-impressions, may very well have been the cause of the difficulty in forming the associations. Nor is it

certain that *ideas* of clawing loops would be easier to form than ideas of scratching or licking oneself. The matter is still open to question. But, as said before, my opinion would be that animals *do* have representations and that such are the beginning of the rich life of ideas in man. For the most part, however, such are confined to specific and narrow practical lines. There was no evidence that my animals habitually *did* form associations of ideas from their experience throughout, or that such were constantly revived without the spur of immediate practical advantage.

Before leaving the topic an account may be given of experiments similar to the one described above as performed on cats 3 and 4, which were undertaken with Cat 13 and Dogs 1, 2 and 3.

Cat 13 was fed with pieces of fish at the top of the wire netting 45 times, to accustom it to climbing up when it saw me come with fish. I then went through the same process as with 3 and 4, but at intervals of 60 to 90 seconds instead of 120. After 90 such trials it occasionally climbed up a little way, but though 135 trials in all were given it never made the uniform and definite reaction which 3 and 4 did. It reacted, when it reacted at all, at from 5 to 9 seconds after the signal. Whether age, weight, lack of previous habitual climbing when I approached, or a slowness in forming the association made the difference, is uncertain.

Dog 1 was experimented on in the following manner: I would put him in a big pen, 20–10 feet, and sit outside facing it, he watching me as was his habit. I would pound with a stick and say, "Go over to the corner." After an interval (10 seconds for 35 trials, 5 seconds for 60 trials) I would go over to the corner (12 feet off) and drop a piece of meat there. He, of course, followed and secured it. On the 6th, 7th, 16th, 17th, 18th and 19th trials he did perform the act before the 10 seconds were up, then for several times went during the two-minute intervals without regarding the signal, and finally abandoned the habit altogether, although he showed by his behavior when the signal was given that he was not indifferent to it.

Dogs 1, 2 and 3 were also given 95, 135 and 95 trials, re-

spectively, the acts done being (1) standing up against the wire netting inclosing the pen, (2) placing the paws on top of a keg, and (3) jumping up onto a box. The time intervals were 5 seconds in each case. No dog of these ever performed the act before I started to take the meat to feed them, but they did show, by getting up if they were lying down, when the signal was given, or by coming to me if they were in some other part of the pen, that something was suggested to them by it. Why these cases differ from the cases of Cats 3 and 4 (10 and 12 also presented phenomena like those reported in the cases of 3 and 4) is an interesting though not very important question. The dogs were not kept so hungry as were the cats, and experience had certainly not rendered the particular impulses involved so sensitive, so ready to discharge. Dogs 2 and 3 were older. There is no reason to invoke any qualitative difference in the mental make-up of the animals until more illuminating experiments are made.

Association by Similarity and the Formation of Concepts.

What there is to say on this subject from the standpoint of my experiments will be best introduced by an account of the experiments themselves.

Dog 1 had escaped from AA (0 at front) 26 times. He was then put in BB (0 at back). Now, whereas 2 and 3, who were put in without previous experience with AA, failed to paw the loop in BB, No. 1 succeeded. His times were 7.00, .35, 2.05, .40, .32, .10, 1.10, .38, .10, .05, and from then on he pawed the loop as soon as put in the box. After a day or so he was put in BB1 (0 at back high). Although the loop was in a new position, his times were only .20, .10, .10, etc. After nine days he was put in a box arranged with a little wooden platform 2½ inches square, hung where the loop was in BB1. Although the platform resembled the loop not the least save in position, his times were only .10, .07, .05, etc.

From the curves given in Figure 19, which tell the history of 10, 11 and 12 in B1 (0 at back) after each had previously been familiarized with A (0 at front), we see this same

influence of practice in reacting to one mechanism upon the time taken to react to a mechanism at all similar. It naturally takes a cat a longer time to accidentally claw a loop in the back than in the front, yet a comparison of these curves with those on page 18, Figure 2, shows the opposite to have been the case with 10, 11 and 12. The same remarkable quickness was noted in Cats 1 and 3 when put into B (o at back) after learning A (o at front). Moreover, the loops were not alike. The loop in A was of smaller wire, covered with a bluish thread, while the loop in B was covered with a black rubber compound, the diameter of the loop being three times that of A's loop.

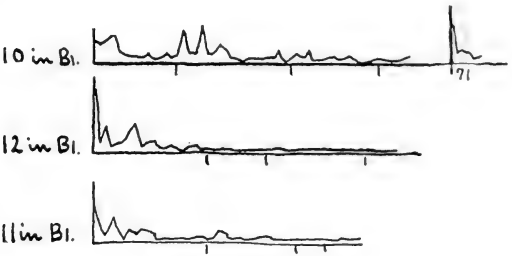

FIG. 19.

If any advocate of reason in animals has read so far, I doubt not that his heart has leaped with joy at these two preceding paragraphs. "How," he will say, "can you explain these facts without that prime factor in human reason, association by similarity? Surely they show the animal perceiving likenesses and acting from general ideas." *This is the very last thing that they show.* Let us see why they do not show this and what they do show. He who thinks that these animals had a general notion of a loop-like thing as the thing to be clawed, that they felt the loop in B, different as it was in size, color and position, to be still a loop, to have the essential quality of the other, must needs presuppose that the cat has a clear, accurate sensation and representation of both. Only if the cat discriminates can it later associate by noticing similarities. This is what such

thinkers do presuppose. A bird, for instance, dives in the same manner into a river of yellow water, a pond or an ocean. It has a general notion, they say, of water. It knows that river water is one thing and pond-water another thing, but it knows that both are water, *ergo*, fit to dive into. The cat who reacts to a loop of small wire of a blue color knows just what that loop is, and when it sees a different loop knows its differences, but knows also its likeness, and reacts to the essential. Thus crediting the cat with our differentiation and perception of individuality, they credit it with our conceptions and perceptions of similarity. Unless the animal has the first there is no reason to suppose the last. Now, *the animal does not have either*. It does not in the first place react to that particular loop in A, with recognition of its qualities. It reacts to a vague, ill-defined sense-impression, undiscriminated and even unperceived in the technical sense of the word. Morgan's phrase, "a bit of pure experience," is perhaps as good as any. The loop is to the cat what the ocean is to a man, when thrown into it when half-asleep. Thus the cat who climbed up the front of the cage whenever I said, "I must feed those cats," would climb up just as inevitably when I said, "My name is Thorndike," or "To-day is Tuesday." So cats would claw at the loop or button when the door was open. So cats would paw at the place where a loop had been, though none was there. The reaction is not to a well-discriminated object, but to a vague situation, and any element of the situation may arouse the reaction. The whole situation in the case of man is speedily resolved into elements; the particular elements are held in focus, and the non-essential is systematically kept out of mind. In the animal the whole situation sets loose the impulse; all of its elements, including the non-essentials, get yoked with the impulse, and the situation may be added to or subtracted from without destroying the association, provided you leave something which will set off the impulse. The animal does not think one is like the other, nor does it, as is so often said, mistake one for the other. It does not think *about* it at all; it just thinks *it*, and the *it* is the kind of "pure experience" we have been describing. In human mental life we have accurate, discriminated sensa-

tions and perceptions, realized as such, and general notions, also realized as such. Now, what the phenomena in animals which we have been considering show is that they have neither. Far from showing an advanced stage of mentality, they show a very primitive and unspecialized stage. They are to be explained not by the presence of *general* notions, but by the absence of notions of *particulars*. The idea that animals react to a particular and absolutely defined and realized sense-impression, and that a similar reaction to a sense-impression which varies from the first proves an association by similarity, is a myth. We shall see later how an animal does come in certain cases to discriminate, in one sense of the word, with a great degree of delicacy, but we shall also see then what must be emphasized now, that naturally the animal's brain reacts very coarsely to sense-impressions, and that the animal does not think *about* his thoughts at all.

This puts a new face upon the question of the origin and development of human abstractions and consequent general ideas. It has been commonly supposed that animals had 'recepts' or such semi-abstractions as Morgan's 'predominants,' and that by associating with these arbitrary and permanent signs, such as articulate sounds, one turned them into genuine ideas of qualities. Professor James has made the simple but brilliant criticism that all a recept really means is *a tendency to react in a certain way*. But I have tried to show that the fact that an animal reacts alike to a lot of things gives no reason to believe that it is conscious of their common quality and reacts to that consciousness, because the things it reacts to in the first place are not the hard-and-fast, well-defined 'things' of human life. What a 'recept' or 'predominant' really stands for is no thing which can be transformed into a notion of a quality by being labelled with a name. This easy solution of the problem of abstraction is impossible. A true idea of the problem itself is better than such a solution.

My statement of what has been the course of development along this line is derived from observations of animals' behavior and Professor James' theory of the nature of and presumable brain processes going with the abstractions and conceptions of

human consciousness, but it is justified chiefly by its harmony with the view that conception, the faculty of having general notions, has been naturally selected by reason of its utility. The first thing is for an animal to learn to react alike only to things which resemble each other in the *essential* qualities. On an artificial, analytic basis, feelings of abstract qualities might grow out of reacting alike to objects similar in such a respect that the reaction would be useless or harmful. But in the actual struggle for existence, starting with the mammalian mind as we have found it, you will tend to get reactions to the *beneficial* similarities by selection from among these so-called mistakes, *before you get any general faculty of noticing similarities*. In order that this faculty of indifferent reaction to different things shall grow into the useful faculty of indifferent reaction to different things *which have all some quality that makes the reaction a fit one*, there must be a tremendous range of associations. For a lot of the similarities which are non-essential have to be stamped out, not by a power of feeling likeness, but by their failure to lead to pleasure. With such a wide range of associations we may get reactions on the one hand where impulses have been connected with one particular sense-impression because when connected with all others they had failed to give pleasure, and on the other hand reactions where an impulse has been connected with numerous different impressions possessing one common quality, and disconnected with all impressions, otherwise like these, which fail to have that one quality.

Combined with this multiplication of associations, there is, I think, an equally important factor, the loosening of the elements of an association from one another and from it as a whole. Probably the idea of the look of the loop or lever or thumb-latch never entered the mind of any one of my cats during the months that they were with me, except when the front end of the association containing it was excited by putting the cat into the box. In general, the unit of their consciousness, apart from impulses and emotions, is a whole association-series. Such soil cannot grow general ideas, for the ideas, so long as they never show themselves except for a particular practical business, will not be thought about or realized in their nature

or connections. If enough associations are provided by a general curiosity, such as is seen among the monkeys, if the mental elements of the association are freed, isolated, felt by themselves, *then* a realization of the ideas, feelings of their similarity by transition from one to the other, feelings of qualities and of meanings, may gradually emerge. Language will be a factor in the isolation of the ideas and a help to their realization. But when anyone says that language has been the cause of the change from brute to man, when one talks as if *nothing but it* were needed turn animal consciousness into human, he is speaking as foolishly as one who should say that a proboscis added to a cow would make it an elephant.

This is all I have to say, in this connection, about association by similarity and conception, and with it is concluded our analysis of the nature of the association-process in animals. Before proceeding to treat of the delicacy, complexity, number and permanence of these associations, it seems worth while to attempt to describe graphically, not by analysis, the mental fact we have been studying, and also to connect our results with the previous theories of association.

One who has seen the phenomena so far described, who has watched the life of a cat or dog for a month or more under test conditions, gets, or fancies he gets, a fairly definite idea of what the intellectual life of a cat or dog feels like. It is most like what we feel when consciousness contains little thought about anything, when we feel the sense-impressions in their first intention, so to speak, when we feel our own body, and the impulses we give to it. Sometimes one gets this animal consciousness while in swimming, for example. One feels the water, the sky, the birds above, but with no thoughts *about* them or memories of how they looked at other times, or aesthetic judgments about their beauty; one feels no *ideas* about what movements he will make, but feels himself make them, feels his body throughout. Self-consciousness dies away. Social consciousness dies away. The meanings, and values, and connections of things die away. One feels sense-impressions, has impulses, feels the movements he makes; that is all.

This pictorial description may be supplemented by an ac-

count of some associations in human life which are learned in the same way as are animal associations; associations, therefore, where the process of formation is possibly homologous with that in animals. When a man learns to swim, to play tennis or billiards, or to juggle, the process is something like what happens when the cat learns to pull the string to get out of the box, provided, of course, we remove, in the man's case, all the accompanying mentality which is not directly concerned in learning the feat.[1] Like the latter, the former contains desire, sense-impression, impulse, act and possible representations. Like it, the former is learned gradually. Moreover, the associations concerned cannot be formed by imitation. One does not know how to dive just by seeing another man dive. You cannot form them from being put through them, though, of course, this helps indirectly, in a way that it does not with animals. One makes use of no feelings of a common element, no perceptions of similarity. The tennis player does not feel, "This ball coming at this angle and with this speed is similar in angle, though not in speed, to that other ball of an hour ago, therefore I will hit it in a similar way." He simply feels an impulse from the sense-impression. Finally, the elements of the associations are not isolated. No tennis-player's stream of thought is filled with free-floating representations of any of the tens of thousands of sense-impressions or movements he has seen and made on the tennis-court. Yet there is consciousness enough at the time, keen consciousness of the sense-impressions, impulses, feelings of one's bodily acts. So with the animals. There is consciousness enough, but of this kind.

[1] A man may learn to swim from the general feeling, "I want to be able to swim." While learning, he may think of this desire, of the difficulties of the motion, of the instruction given him, or of anything which may turn up in his mind. This is all extraneous and is not concerned in the acquisition of the association. Nothing like it, of course, goes on in the animal's mind. Imagine a man thrown into the water repeatedly, and gradually floundering to the shore in better and better style until finally, when thrown in, he swims off perfectly, and deprive the man of all extraneous feelings, and you have an approximate homologue of the process in animals. He feels discomfort, certain impules to flounder around, some of which are the right ones to move his body to the shore. The pleasure which follows stamps in these and gradually the proper movements are made immediately on feeling the sense-impression of surrounding water.

Thus, the associations in human life, which compare with the simple connections learned by animals, are associations involving connections between novel, complex, and often inconstant sense-impressions and impulses to acts similarly novel, complex and often inconstant. Man has the elements of most of his associations in isolated form, attended to separately, possessed as a permanent fund, recallable at will, and multifariously connected among themselves, but with these associations which we have mentioned, and with others like them, he deals as the animals deal with theirs. The process, in the man's mind, leaving out extraneous mental stuff, may be homologous to the association-process in animals. Of course, by assiduous attention to the elements of these associations, a man may isolate them, may thus get these associations to the same plane as the rest. But they pass through the stage we have described, even then, and with most men, stay there. The abstraction, the naming, etc., generally come from observers of the game or action, and concern things as felt by them, not by the participant.

Criticism of Previous Theories.

We may now look for a moment at what previous writers have said about the nature of association in animals. The complaint was made early in this book that all the statements had been exceedingly vague and of no value, except as retorts to the 'reason' school. In the course of the discussion I have tried to extricate from this vagueness definite statements about imitation, association of ideas, association by ideas. There is one more theory, more or less hidden in the vagueness: the theory that association in animals is the same as association in man, that the animal mind differs from the human mind only by the absence of reason and what it implies. Presumably, silence about what association is, means that it is the association which human psychology discusses. When the silence is broken, we get such utterances of this theory as the following:

"I think we may say then that the higher animals are able to proceed a long way in the formation and definition of highly complex constructs, analogous to but probably differing some-

what from those which we form ourselves. These constructs, moreover, through association with reconstructs or representations, link themselves in trains so that a sensation, or group of sensations, may suggest a series of reconstructs, or a series of remembered phenomena." (C. L. Morgan, 'Animal Life and Intelligence,' p. 341.)

"Lastly, before taking leave of the subject of the chapter, I am most anxious that it should not be thought that, in contending that intelligence is not reason, I wish in any way to disparage intelligence. Nine-tenths at least of the actions of average men are intelligent and not rational. Do we not all of us know hundreds of practical men who are in the highest degree intelligent, but in whom the rational, analytic faculty is but little developed? Is it any injustice to the brutes to contend that their inferences are of the same order as those of these excellent practical folk? In any case, no such injustice is intended; and if I deny them self-consciousness and reason I grant to the higher animals perceptions of marvellous acuteness and intelligent inferences of wonderful accuracy and precision—intelligent inferences in some cases, no doubt, more perfect even than those of man, who is often disturbed by many thoughts" (ibid., p. 376–377).

"Language and the analytic faculty it renders possible, differentiate man from the brute" (ibid., p. 374).

Here, as elsewhere, it should be remembered that Lloyd Morgan is not quoted because he is the worst offender or because he represents the opposite in general of what the present writer takes to be the truth. On the contrary, Morgan is quoted because he is the least offender, because he has taken the most advanced stand along the line of the present investigation, because my differences from him are in the line of his differences from other writers. With the theory of the passages just quoted, however, which attribute extensive association of ideas and general powers comparable to those of men minus reason, to the brutes, and which repeat the time-honored distinction by language, I do not, in the least, agree. Association in animals does not equal association in man. The latter is built over and permeated and transformed by inference and

judgment and comparison; it includes imitation in our narrow sense of transferred association; it obtains where no impulse is included; it thus takes frequently the form of long trains of thought ending in no pleasure-giving act; its elements are often loose, existing independently of the particular association; the association is not only thought, but at the same time thought *about*. None of these statements may be truthfully made of animal association. Only a small part of human association is at all comparable to it. My opinion of what that small part is has already been given. Moreover, further differences will be found as we consider the data relating to the delicacy, complexity, number, and permanence of associations in animals. I said a while ago that man was no more an animal with language than an elephant was a cow with a proboscis. We may safely broaden the statement and say that *man is not an animal plus reason*. It has been one great purpose of this investigation to show that even after leaving reason out of account, there are tremendous differences between man and the higher animals. The problem of comparative psychology is not only to get human reason from some lower faculties, but to get human *association* from animal association.

Our analysis, necessarily imperfect because the first attempted, of the nature of the association-process in animals is finished and we have now to speak of its limitations in respect to delicacy, complexity, number and permanence.

Delicacy of Associations.

It goes without saying that the possible delicacy of associations is conditioned by the delicacy of sense-powers. If an animal doesn't feel differently at seeing two objects, it cannot associate one with one reaction, the other with another. An equally obvious factor is attention; what is not attended to will not be associated. Beyond this there is no *a priori* reason why an animal should not react differently to things varying only by the most delicate difference, and I am inclined to think an animal could; that any two objects with a difference appreciable by sensation which are also able to win attention may be reacted to differently. Experiments to show this are very tedious

and the practical question is, "What will the animal naturally attend to?" The difficulty, as all trainers say, is to get the animal's attention to your signal somehow. Then he will in time surely react differently, if you give him the chance, to a figure 7 on the blackboard from the way he does to a figure 8, to your question, "How many days are there in a week?" and to your question, "How many legs have you?" The chimpanzee in London that handed out 3, 4, 5, 6 or 7 straws at command was not thereby proved of remarkable intelligence or of remarkably delicate associative power. Any reputable animal trainer would be ashamed to exhibit a horse who could not do as much 'counting' as that. The maximum of delicacy in associating exhibited by any animal, to my knowledge, is displayed in the performance of the dog 'Dodgerfield,' exhibited by a Mr. Davis, who brings from four cards, numbered 1, 2, 3 and 4, whichever one his master shall *think of*. That is, you write out an arbitrary list, *e. g.*, 4, 2, 1, 3, 3, 2, 2, 1, 4, 2, etc., and hand it to Mr. Davis, who looks at the list, thinks of the first number, says "Attention! Dodger!" and then, "Bring it." This the dog does and so on through the list. Mr. Davis makes no signals which anyone sitting even right beside or in front of him can detect. Thus the dog exceeds the human observers in delicacy and associates each with a separate act four attitudes of his master, which to human observers seem all alike. Mr. Davis says he thinks the dog is a mind-reader. I think it quite possible that whatever signs the dog goes by are given unconsciously and consist only of some very delicate general differences in facial expression or the manner of saying the words, "Bring it," or slight sounds made by Mr. Davis in thinking to himself the words one or two or three or four. Mr. Davis keeps his eyes shut and his hands behind a newspaper. The dog looks directly at his face.

To such a height possible delicacy may attain, but possible delicacy is quite another thing from actual untrained and unstimulated delicacy. The difference in reaction has to be brought about by associating with pleasure the reaction to the different sense-impression when it itself differs and associating with pain tendencies to confuse the reactions. The animal does

not naturally as a function of sense-powers discriminate at all delicately. Thus the cat who climbed up the wire netting when I said, "I must feed those cats!" did not have a delicate association of just that act with just those words. For after I had dropped the clapping part of the signal and simply used those words it would react just as vigorously to the words, "To-morrow is Tuesday" or "My name is Thorndike." The reaction naturally was to a very vague stimulus. Taking cat 10 when just beginning to learn to climb up at the signal, "I must feed those cats!" I started in to improve the delicacy, by opposing to this formula the formula, "I will not feed them," after saying which, I kept my word. That is, I gave sometimes the former signal and fed the cat, sometimes the latter and did not. The object was to see how long the cat would be in learning always to go up when I gave the first, never to do so when I gave the second signal. I said the words in both cases as I naturally would do, so that there was a difference in emphasis and tone as well as in the mere nature of the syllables. The two signals were given in all sorts of combinations so that there was no regularity in the recurrence of either which might aid the animal. The cat at first did not always climb up at the first signal and often *did* climb up at the wrong one. The change from this condition to one of perfect discrimination is shown in the accompanying curves, one showing the decrease in *failures* to respond to the right signal, the other showing the decrease in responses to the wrong signal. The first curve is formed by a line joining the tops of perpendiculars erected at intervals of 1 mm. along the abscissa. The height of a perpendicular represents the number of times the cat failed to respond to the food-signal in 20 trials, a height of 1 mm. being the representative of one failure. Thus, the entire curve stands for 280 trials, there being no failures after 60 trials, and only 1 after the 40th.

In the other curve, also, each 1. mm. along the abscissa stands for 20 trials, and the perpendiculars whose tops the curve unites represent the number of times the cat in each 20 *did* climb up at the signal which meant no food. It will be seen that 380 experiences were necessary before the animal learned that the second signal was different from the first. The

experiment shows beautifully the animal method of acquisition. If at any stage the animal could have isolated the two ideas of the two sense-impressions, and felt them together in comparison, this long and tedious process would have been unnecessary.

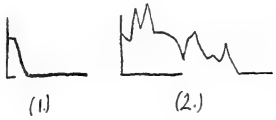

FIG. 20.

It might be stated here that the animals also acquired associations of moderate delicacy in discriminating between the different boxes. No cat tried to get out of A or B by licking herself, for instance.

The question may naturally be raised that if naturally associations are thus vague, the common phenomenon of a dog obeying his master's commands, and no one else's, is inexplicable. The difference between one man and another, one voice and another, it may be said, is not much of a difference, yet is here uniformly discriminated, although we cannot suppose any such systematic training to reject the other slightly differing commands. My cats did not so discriminate. If anyone else sat in my chair and called out, "I must feed the cats," they reacted, and probably very many animals would, if untroubled by emotions of curiosity or fear at the new individual, go through their tricks as well at another's voice as at that of their master. The other cases exemplify the influence of attention. Repeated attention to these sense-impressions has rendered them clear-cut and detailed, and the new impression consequently does not equal them in calling forth the reaction.

The main thing to carry away from this discussion is the assurance that the delicacy of the animal in associating acts with impressions is nothing like the delicacy of the man who feels that a certain tone is higher, or weight is heavier, than another, but *is* like the delicacy of the man who runs to a certain spot to hit one tennis ball and to a different spot to hit one coming with a slightly different speed.

Complexity of Associations.

An important question, especially if one wishes to rate an animal in a scale of intelligence, is the question of how complex an association it can form. A man can learn that to open a door he has to put the key in its hole, turn it, turn the knob, and pull the door. Here, then, is a complex act connected with the simple sense-impression. Or, conversely, a man knows that when the ringing of a bell is followed by a whistle and that by a red light he is to do a certain thing, while if any of the three happen alone he is not to. How far, then, we ask, can animals go along the line of increased complexity in the associations?

We must not mistake for a complex association a series of associations, where one sense-impression leads to an act such as to present a new sense-impression which leads to another act which in its turn leads to a new sense-impression. Of the formation of such *series* animals are capable to a very high degree. Chicks from 10 to 25 days old learned to go directly through a sort of big labyrinth requiring a series of 23 distinct and in some cases fairly difficult associations, of which 11 involved choices between two paths. By this power of acquiring a long series animals find their way to distant feeding grounds and back again. But all such cases are examples of the *number*, not of the complexity, of animal associations.

Some of my boxes were such as did give a chance for a complex association to be formed. Such were G (*thumb-latch*), J (*double*), K and L (triples) for the cats, and O (triple) for the dogs. It would be possible for a cat, after stepping on the platform in K, to notice that the platform was in a different position, and so feel then a different sense-impression from before, and thus turn the thing into a serial association. The cat would then be like a man who on seeing a door should feel only the impulse to stick the key in the hole, but then, seeing the door plus a key in the hole, should feel the impulse to turn the key and so on through. My cats did not give any signs of this, so that with them it was either a complex association or an irregular happening of the proper impulses. Probably the same was the case with dog 1. Cats 10, 11, 12 in L knew all the movements separately before being experimented on with the

combination. Cats 2, 3, 4, had had some experience of D, which worked by a string something like the string part of K. The string in K was, however, quite differently situated and required an altogether different movement to pull it. Since further No. 2, who had had ten times as much experience in D as 3 or 4, succeeded no better with the string element of K than they, it is probable that the experience did not help very much. All else in all these compound associations was new. At the same time the history of these animals' dealings with these boxes would not fairly represent that of animals without general experience of clawing at all sorts of loose or shaky things in the inside of a box. These cats had learned to claw at all sorts of things. The time curves were taken as in the formation of the other associations, and, in addition, the order in which the animal did the several things required was recorded in every trial.

In the case of all the curves, except the latter part of 3 in G, one notices a very gradual slope and an excessive irregularity in the curve throughout. Within the limits of the trials given the animals are unable to form a perfect association and what advancement they make is very slow. The case of 3 in G is not an exception to this, but a proof of it. For 3 succeeded in making a perfect association, by accidentally hitting on a way to turn the compound association into a simple one. He happened one time to paw down the thumb-piece at the same time that his other fore-limb, with which he was holding on between the door and the top of the box, was pressing against the door. This giving him success he repeated it in later trials and in a short time had it fixed as an element in a perfect association. The marked change in his curve from an irregular and gradual slope at such a height as displayed a very imperfect association to a constant and very slight height shows precisely the change from a compound to a simple association.

Compound associations are formed slowly and not at all well. Further observation shows that they were really not formed at all. For the animals did not, except 3 in K for a certain period, do the several things in a constant order, nor did they do them only once apiece. On the contrary, an animal would pull the string several times after the bolt had gone up with its custom-

ary click, and would do sometimes one thing first, sometimes another. It may also be noted here, in advance of its proper place, that these compound associations are far below the simple in point of permanence. The conduct of the animals is clearly not that of minds having associated with a certain box's interior the idea of a succession of three movements. The animal does not feel, "I did this and that and that and got out," or, more simply still, "this and that and that means getting out." If it did we should soon see it doing what was necessary without repetition and in a fairly constant time.

I imagine, however, that an animal could learn to associate with one sense-impression a compound act so as to perform its elements in a regular order. By arranging the box so that the second and third elements of the act could be performed *only after the first had been*, and the third *only after the first and second*, I am inclined to think you could get a very vigorous cat to learn the elements in order and form the association perfectly. The case is comparable to that of delicacy. The cat does not *tend* to know what he is doing or to depart from the hit-or-miss method of learning, but by associating the other combinations of elements with failure to get pleasure, as in delicacy experiments we associated the reactions to all but the one signal, you could probably stamp out all but the 1, 2, 3 order.

The fact that you have to thus manœuvre to get the animals to have the three impulses in a regular order shows that even when they are so there is no idea of the three as in an order, no thinking about them. Representations do not get beyond their first intention. They are not carried up into a free life which works them over anew. A complex *act* does not imply a complex *thought*, or, more exactly, a performance of a series does not imply the thought of a series. Consequently, since the complexity of the act depends on the power which failure has to stamp out all other combinations, it is far more limited than in man.

Number of Associations.

The patent and important fact is that there are so few in animals compared to the human stock. Even after taking into

account the various acts associated with various smells, and exaggerating the possibility of getting an equipment of associations in this field which man lacks, one must recognize how far below man any animal is in respect to mere quantity of asssociations. The associations with words alone of an average American child of ten years far outnumber those of any dog. A good billiard-player probably has more associations in connection with this single pastime than a dog with his whole life's business. In the associations which are homologous with those of animals man outdoes them and adds an infinity of associations of a different sort. The primates would seem, by virtue of their incessant curiosity and addition to experience not for any practical purpose but merely for love of mental life, to represent an advanced stage toward this tremendous quantity of associations. In man not only this activity and curiosity, but also education, increases the number of associations. Associations are formed more quickly, and the absence of need for self-support during a long infancy gives time. Associations thus formed work back upon practical life, and by showing better ways decrease the need of work, and so again increase the chance to form associations. The result in the case of a human mind to-day is the possession of a thesaurus of valuable associations, if the time has been wisely spent. The free life of ideas, imitation, all the methods of communication, and the original accomplishments which we may include under the head of invention, make the process of acquisition in many cases quite a different one from the trial and error method of the animals, and in general much shorten it.

Small as it is, however, the number of associations which an animal may acquire is probably much larger than popularly supposed.

My cats and dogs did not mix up their acts with the wrong sense-impressions. The chicks that learned the series of twenty-three associations did not find it a task beyond their powers to retain them. Several three-day-old chicks, which I caused to learn ten simple associations in the same day, kept the things apart and on the next morning went through each act at the proper stimulus. In the hands of animal trainers some animals

get a large number of associations perfectly in hand. The horse Mascot is claimed to know the meaning of fifteen hundred signals! He certainly knows a great many, and such as are naturally difficult of acquisition. It would be an enlightening investigation if some one could find out just how many associations a cat or dog could form, if he were carefully and constantly given an opportunity. The result would probably show that the number was limited only by the amount of motive available and the time taken to acquire each. For there is probably nothing in their brain structure which limits the number of connections that can be formed, or would cause such connections, as they grew numerous, to become confused.

In their anxiety to credit animals with human powers, the psychologists have disregarded or belittled, perhaps, the possibilities of the strictly animal sort of association. They would think it more wonderful that a horse should respond differently to a lot of different numbers on the black-board than that he should infer a consequence from premises. But if it be made a direct question of pleasure or pain to an animal, he can associate any number of acts with different stimuli. Only he does not form any association until he has to, until the direct benefit is apparent, and, for his ordinary life, comparatively few are needed.

On the whole our judgment from a comparison of man's associations with the brutes' must be that man's are naturally far more delicate, complex and numerous, and that in as far as the animals attain delicacy, complexity, or a great number of associations, they do it by methods which man uses only in a very limited part of the field.

Permanence of Associations.

Once formed, the connections by which, when an animal feels a certain sense-impression, he does a certain thing, persist over considerable intervals of time. With the curves on pages 18 to 26 and 33 to 34 are given in many instances[1] additional

[1] See 10 in A, 3 in A, 10 in D; 10 in C, 4 in C, 3 in C; 6, 2, 5, 4 in E; 4 in F; 10 in H, 3 in H; 3, 4, 5, in I; 4 in G, 3 in G; 3 in K; 10 in L; dog 1 in N and CC; dog 1 in G and O.

curves showing the animal's proficiency after an interval without experience. To these data may be added the following:

The three chicks that had learned to escape through the long labyrinth (involving twenty-three associations) succeeded in repeating the performance after ten days' interval. Similarly the chicks used as imitators in V, W, X and Y, did not fail to perform the proper act after an interval of twenty days. Cat 6, who had had about a hundred experiences in C *(Button)*, had the association as perfect after twenty days as when it left off. Cat 2, who had had 36 experiences with C and had attained a constant time of 8 sec., escaped fourteen days later in 3, 9 and 8 secs. respectively, in three trials. Cat 1, after an interval of twenty days, failed in 10 minutes to escape from C. The signal for climbing up the front of the cage was reacted to by No. 3 after an interval of twenty-four days. No. 10, who had learned to discriminate between 'I must feed those cats' and 'I will not feed them,' was tried after *eighty days*. It was given 50 trials with the second signal mingled indiscriminately with 25 trials with the first. I give the full record of these, 'yes' equalling a trial in which she 'forgot' and climbed up, 'no' equalling a trial in which she wisely stayed down. Dashes represent intervening trials with the first signal, *to which she always reacted.* It will be observed that 50 trials put the cat in the same position that 350 had done in her first experience, although in that first experience she had had only about a hundred trials after the association had been perfected. The association between the first signal and climbing up was perfect after the eighty days.

Trials 1-7.	Trials 8-17.	Trials 18-27.	Trials 28-35.	Trials 36-42.	Trials 43-50.
——	yes	no	——	——	——
——	yes	yes	——	no	——
yes	yes	no	——	no	——
yes	——	no	no	——	——
no	yes	——	no	no	——
——	yes	——	yes	no	no
yes	no	yes	no	no	no
yes	yes	yes	——	——	yes
no	no	yes	no	——	no
no	——	yes	yes	no	no
——	——	no	no	no	no
——	yes	no	no		no
——	yes				no
	——				

All these data show that traces of the connections once formed are very slow in being lost. If we allow that part of the time in the first trial in all these cases is due to the time taken to realize the situation (time not needed in the trials when the association is forming and the animal is constantly being dropped into boxes) we may say that the association is as firm as ever for a considerable time after practice at it is stopped. How long a time would be required to annul the influence of any given quantity of experience, say of an association which had been gone through with ten times, I cannot say. It could, if profitable, easily be determined in any case. The only case of total loss of the association (No. 1 in C) is so exceptional that I fancy something other than lapse of time was its cause. The main interest of these data, considered as quantitative estimates, is not psychological, but biological. They show what a tremendous advantage the well-developed association-process is to an animal. The ways to different feeding grounds, the actions of enemies, the appearance of noxious foods, are all connected permanently with the proper reaction by a few experiences which need be reinforced only very rarely. Of course, associations without any permanence would be useless, but the usefulness increases immensely with such a degree of permanence as these results witness. An interesting experiment from the biological point of view would be to see how infrequently an experience could occur and yet lead eventually to a perfect association. An experiment approximating this is recorded in the time-curves for Box H in Fig. 4, on page 20. Three trials at a time were given, the trials being two or three days apart. As may be seen from the curves, the association was readily formed.

The chief psychological interest of these data is that they show that permanence of associations *is not memory*. The fact that a cat, when after an interval she is put into box G, proceeds to immediately press the thumb-piece and push the door, does not at all mean that the cat feels the box to be the same from which she weeks ago freed herself by pushing down that thumb-piece, or thinks about ever having felt or done anything in that box. She does not refer the present situation to a situation

of the past and realize that it is the same, but simply feels on being confronted with that situation the same impulse which she felt before. She does the thing now for just the same reason that she did it before, namely, because pleasure has connected that act above all others with that sense-impression, so that it is the one she feels like doing. Her condition is that of the swimmer who starts his summer season after a winter's deprivation. When he jumps off the pier and hits the water he swims, not because he remembers that this is the way he dealt with water last summer and so applies his remembrance to present use, but just because experience has taught him to feel like swimming when he hits the water. All talk about recognition and memory in animals, if it asserts the presence of anything more than this, is a gross mistake. For real memory is an absolute thing, including everything but forgetfulness. If the cat had real memory it would, when after an interval dropped into a box, remember that from this box it escaped by doing this or that and consequently, either immediately or after a time of recollection, go do it, or else it would not remember and would fail utterly to do it. On the contrary, we have all grades of *partial* 'forgetfulness,' just like the grades of swimming one might find if he dropped a dozen college professors into the mill-ponds of their boyhood, just like the grades of forgetfulness of the associations once acquired on the ball-field which are manifested when on the Fourth of July the 'solid men' of a town get out to amuse their fellow citizens. The animal makes attacks on a spot around the vital one, or claws at the thing—but not so precisely as before, or goes at it a while and then resorts to instinctive methods of getting out. Its actions are exactly what would be expected of an animal in whom the sense-impression aroused the impulse imperfectly, or weakly, or intermittently, but are not at all like the actions of one who felt, "I used to get out of this box by pulling that loop down." In fact, the record of No. 10 given on page 96 seems to be final on this point. If at any time in the course of the 50 trials it had *remembered* that 'I will not feed them' meant 'no fish,' it would thenceforth have failed to react. It would have stopped short in the 'yes' reactions, instead of gradually decreasing their percentage. 'Memory'

in animals, if one still chooses to use the word, is *permanence of associations*, not the presence of an idea of an experience attributed to the past.

To this proposition two corollaries may be added. First, these phenomena of incomplete forgetfulness extend the evidence that animals do not have a stock of independent ideas, the return of which plus past associates equals memory. Second, there is, properly speaking, no continuity in their mental streams. The present thought does not clutch the past to its bosom or hold the future in its womb. The animal's self is not a being 'looking before and after,' but a direct practical association of feelings and impulses. So far as experiences come continuously they may be said to form a continuous mental life, but there is no continuity imposed from within. The feelings of its own body are always present and impresssions from outside may come as they come to us. When the habit of attending to the elements of its associations and raising them up into the life of free ideas is acquired, these permanent bodily associations may become the basis of a feeling of self-hood and the trains of ideas may be felt as a continuous life.

Inhibition of Instincts by Habit.

One very important result of association remains to be considered, its inhibition of instincts and previous associations. An animal who has become habituated to getting out of a box by pulling a loop and opening the door will do so even though the hole in the top of the box be uncovered, whereas, if, in early trials, you had left any such hole, he would have taken the instinctive way and crawled through it. Instances of this sort of thing are well-nigh ubiquitous. It is a tremendous factor in animal life, and the strongest instincts may thus be annulled. The phenomenon has been already recognized in the literature of the subject, a convenient account being found in James' *Psychology*, Vol. II., pages 394–397. In addition to such accounts, one may note that the influence of association is exerted in two ways. The instinct may wane by not being used, because the animal forms the habit of meeting the situation in a different way, or it may be actually inhibited. An instance

of the former sort is found in the history of a cat which learns to pull a loop and so escape from a box whose top is covered by a board nailed over it. If, after enough trials, you remove a piece of the board covering the box, the cat, when put in, will still pull the loop instead of crawling out through the opening thus made. But, at any time, if she happens to notice the hole, she *may* make use of it. An instance of the second sort is that of a chick which has been put on a box with a wire screen at its edge, preventing her from jumping directly down, as she would instinctively do, and forcing her to jump to another box on one side of it and thence down. In the experiments which I made, the chick was prevented by a second screen from jumping directly from the second box also. That is, if in the accompanying figure, A is a box 34 inches high, B a box 25 inches

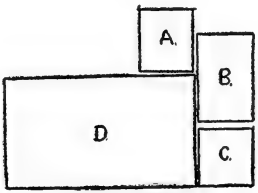

FIG. 21.

high, C a box 16 inches high, and D the pen with the food and other chicks, the subject had to go A–B–C–D. The chick tried at first to get through the screen, pecked at it and ran up and down along it, looking at the chicks below and seeking for a hole to get through. Finally it jumped to B and, after a similar process, to C. After enough trials it forms the habit and when put on A goes immediately to B, then to C and down. Now if, after 75 or 80 trials, you take away the screens, giving the chick a free chance to go to D from either A or B, and then put it on A, the following phenomenon appears. The chick goes up to the edge, looks over, walks up and down it for a while, still looking down at the chicks below, and then goes and jumps to B as habit has taught it to do. The same actions take place on B. No matter how clearly the chick sees the

chance to jump to D, it does not do so. The impulse has been truly inhibited. It is not the mere habit of going the other way, but the impossibility of going *that* way. In one case I observed a chick in whom the instinct was all but, yet not quite inhibited. When tried without the screen, it went up to the edge to look over *nine times*, and at last, after seven minutes, did jump straight down.

Attention.

I have presupposed throughout one function which it will be well to now recognize explicitly—attention. As usual, attention emphasizes and facilitates the process which it accompanies. Unless the sense-impression is focussed by attention, it will not be associated with the act which comes later. Unless two differing boxes are attended to, there will be no difference in the reactions to them. The really effective part of animal consciousness, then, as of human, is the part which is attended to; attention is the ruler of animal as well as human mind.

But in giving attention its deserts we need not forget that it is not here comparable to the whole of human attention. Our attention to the other player and the ball in a game of tennis *is* like the animal's attention, but our attention to a passage in Hegel, or the memory which flits through our mind, or the song we hear, or the player we idly watch, is *not*. There ought, I think, to be a separate name for attention when working for immediate practical associations. It is a different species from that which holds objects so that we may define them, think about them, remember them, etc., and the difference is, as our previous sentence shows, not that between voluntary and involuntary attention. The cat watching me for signs of my walking to the cage with fish is not in the condition of the man watching a ball game, but in that of the player watching the ball speeding toward him. There is a notable difference in the permanence of the impression. The man watching the game can remember just how that fly was hit and how the fielder ran for it, though he bestowed only a slight quantity of attention on the matter, while the fielder may attend to the utmost to the ball and yet not remember at all how it came or how he ran for

it. The one sort of attention leads you to *think* about a thing, the other to *act* with reference to it. We must be careful to remember that when we say that the cat attended to what was said, we do not mean that he thereby established an idea of it. Animals are not proved to form separate ideas of sense-impressions because they attend to them, for the kind of attention they give is the kind which when given by men results in practical associations, not in establishing ideas of objects. If attention rendered clear the idea, we should not have the phenomena of incomplete forgetfulness lately mentioned. The animal would get a definite idea of just the exact thing done and would do it or nothing. The human development of attention is in closest connection with the acquisition of a stock of free ideas.

Social Consciousness.

Besides attention there is another topic somewhat apart from our general one, which yet deserves a few words. It concerns animals' social consciousness, their consciousnes of the feelings of their fellows. Do animals, for example, when they see others feeding, feel that the others are feeling pleasure? Do they, when they fight, feel that the other feels pain? So level-headed a thinker as Lloyd Morgan has said that they do, but the conduct of my animals would seem to show that they did not. For it has given us good reason to suppose that they do not possess *any* stock of isolated ideas, much less any abstracted, inferred or transferred ideas. These ideas of others' feelings imply a power to transfer states felt in oneself to another and realize them as there. Now it seems that any ability to thus transfer and realize an idea ought to carry with it an ability to form a transferred association, to imitate. If the animal realizes the mental states of the other animal who before his eyes pulls the string, goes out through the door and eats fish, he ought to form the association, 'impulse to pull string, pleasure of eating fish.' This we saw the animal could not do.

In fact, pleasure in another, pain in another, is not a sense-presentation or a representation or feeling of an object of any sort, but rather a 'meaning,' a feeling '*of the fact that.*' It can exist only as something thought *about*. It is never 'a bit of

direct experience,' but an abstraction from our own life referred to that of another.

I fancy that these feelings of others' feelings may be connected pretty closely with imitation, and for that reason may begin to appear in the monkeys. There we have some fair evidence for their presence in the tricks which monkeys play on each other. Such feelings seem the natural explanation of the apparently useless tail-pullings and such like which make up the attractions of the monkey-cage. These may, however, be instinctive forms of play-activity or merely examples of the general tendency of the monkeys to fool with everything.

Interaction.

I hope it will not be thought impertinent if from the standpoint of this research I add a word about a general psychological problem, the problem of interaction. I have spoken all along of the connection between the situation and a certain impulse and act being stamped in when pleasure results from the act and stamped out when it doesn't. In this fact, which is undeniable, lies a problem which Lloyd Morgan has frequently emphasized. *How are pleasurable results able to burn in and render predominant the association which led to them?* This is perhaps the greatest problem of both human and animal psychology. Unfortunately in human psychology it has been all tangled up with the problems of free will, mental activity, voluntary attention, the creation of novel acts, and almost everything else. In our experiments we get the data which give rise to the problem, in a very elementary form.

It should first be noted about the *fact* that the pleasure does not burn in an impulse and act themselves, but an impulse and act *as connected with that particular situation*. No cat ever goes around clawing, clawing, clawing all the time, because clawing in these boxes has resulted in pleasure. Secondly, the connection thus stamped in is *not contemporaneous, but prior to* the pleasure. So much for the fact; now for the explanation. I do not wish to rehearse or add to the arguments with which so many pages have been already filled by scientists and philosophers both. What we need most

is not argument, but accurate accounts of the mental fact and of the brain-process. But I do wish to say to the parallelist, what has not to my knowledge been said, that if he presupposes, to account for this fact, a 'physical analogue of the hedonic consciousness,' it is his bounden duty to first show how any motion in any neuron or group of neurons in the nervous system can possess this power of stamping in any current which causes it. For no one would, from our present knowledge of the brain, judge *a priori* that any motion in any part of it could be conceived which should be thus regnant over all the others. And next he must show the possibility of the current which represents the association being the excitant of this regnant motion in a manner direct enough for the purpose.

I wish also to say that whoever thinks that, going along with the current which parallels the association, there is an accompanying minor current, which parallels the pleasure and which stamps in the first current when present with it, flies directly in the face of the facts. *There is no pleasure along with the association. The pleasure does not come until after the association is done and gone.* It is caused by no such minor current, but by the excitation of peripheral sense-organs when freedom from confinement is realized or food is secured. Of course, the notion of such a secondary sub-current is mythology, any way.

To the interactionist I would say: "Do not any more repeat in tiresome fashion that consciousness *does* alter movement, but get to work and show when, where, in what forms and to what degrees it does so. Then, even if it turns out to have been a physical parallel that did the work, you will, at least, have the credit of attaining the best knowledge about the results and their conditions, even though you misnamed the factor."

Besides this contribution to general psychology, I think we may safely offer one to pedagogical science. At least some of our results possess considerable pedagogical interest. The fundamental form of intellection, the association-process in animals, is one, we decided, which requires the personal experience of the animal in all its elements. The association cannot be taught by putting the animal through it or giving it a chance to imitate. Now every observant teacher realizes how often the cleverest

explanation and the best models for imitation fail. Yet often, in such cases, a pupil, if somehow enticed to do the thing, even without comprehension of what it means, even without any real knowledge of what he is doing, will finally get hold of it. So, also, in very many kinds of knowledge, the pupil who does anything from imitation, or who does anything from being put through it, fails to get a real and permanent mastery of the thing. I am sure that with a certain type of mind the only way to teach fractions in algebra, for example, is to get the pupil to do, do, do. I am inclined to think that in many individuals certain things cannot be learned save by actual performance. And I think it is often a fair question when explanation, imitation, and actual performance are all possible methods, which is the best. We are here alongside the foundations of mental life, and this hitherto unsuspected law of animal mind may prevail in human mind to an extent hitherto unknown. The best way with children may often be in the pompous words of an animal trainer, 'to arrange everything in connection with the trick so that the animal will be compelled by the laws of his own nature to perform it.'

This does not at all imply that I think, as a present school of scientists seem to, that because a certain thing *has been* in phylogeny we ought to repeat it in ontogeny. Heaven knows that Dame Nature herself in ontogeny abbreviates and skips and distorts the order of the appearance of organs and functions, and for the best of reasons. We ought to make an effort, as she does, to omit the useless and antiquated and get to the best and most useful as soon as possible; we ought to change what *is* to what *ought to be*, as far as we can. And I would not advocate this animal-like method of learning in place of the later ones unless it does the same work better. I simply suggest that in many cases where at present its use is never dreamed of, it may be a good method. As the fundamental form of intellection every student of *theoretical* pedagogy ought to take it into account.

There is one more contribution, this time to anthropology. If the method of trial and error, with accidental success, be the method of acquiring associations among the animals, the slow

progress of primitive man, the long time between stone age and iron age, for instance, becomes suggestive. Primitive man probably acquired knowledge by just this process, aided possibly by imitation. At any rate, progress was not by seeing through things, but by accidentally hitting upon them. Very possibly an investigation of the history of primitive man and of the present life of savages in the light of the results of this research might bring out old facts in a new and profitable way.

Comparative psychology has, in the light of this research, two tasks of prime importance. One is to study the passage of the child-mind from a life of immediately practical associations to the life of free ideas; the other is to find out how far the anthropoid primates advance toward a similar passage, and to ascertain accurately what faint beginnings or preparations for such an advance the early mammalian stock may be supposed to have had. In this latter connection I think it will be of the utmost importance to bear in mind the possibility that *the present anthropoid primates may be mentally degenerate*. Their present aimless activity and incessant, but largely useless, curiosity may be the degenerated vestiges of such a well-directed activity and useful curiosity as led *homo sapiens* to important practical discoveries, such as the use of tools, the art of making fire, etc. It is even a remote possibility that their chattering is a *relic* of something like language, not a *beginning* of such. Comparative psychology should use the phenomena of the monkey-mind of to-day to find out what the primitive mind from which man's sprung off was like. That is the important thing to get at, and the question whether the present monkey-mind has not gone back instead of ahead is an all-important question. A natural and perhaps sufficient cause of degeneracy would be arboreal habits. The animal that found a means of survival in his muscles might well lose the means before furnished by his brain.

To these disconnected remarks still another must be added, addressed this time to the anecdote school. Some member of it who has chanced to read this may feel like saying: "This experimental work is all very well. Your cats and dogs represent, it is true, specimens from the top stratum of animal intelli-

gence, and your negations, based on their conduct, may be authoritative so far as concerns the average, typical mammalian mind. But our anecdotes do not claim to be stories of the conduct of the average or type, but of those exceptional individuals who have begun to attain higher powers. And, if even a few dogs and cats have these higher powers, our contention is, in a modified form, upheld." To all this I agree, provided the anecdote school now realize just what sort of a position they hold. They are clearly in pretty much the same position as spiritualists. Their anecdotes are on pretty much the same level as the anecdotes of thought-transference, materializations of spirits, super-normal knowledge, etc. Not in quite the same position, for far greater care has been given by the Psychical Research Society to establishing the criteria of authenticity, to insuring good observation, to explaining by normal psychology all that can be so explained, in the case of the latter than the anecdote school has done in the case of the former. The off-hand explanation of certain anecdotes by invoking reason, or imitation, or recognition, or feelings of qualities, is on a par with the explanation of trance-phenomena and such like by invoking the spirits of dead people. I do not deny that we may get lawfully a super-normal psychology, or that the super-normal acts it finds may turn out to be explained by these functions which I have denied to the normal animal mind. But I must soberly declare that I think there is less likelihood that such functions are the explanation of animal acts than that the existence of the spirits of dead people is the true explanation of the automatisms of spiritualistic phenomena. So much for the anecdote school, if it calls itself by its right name and pretends only to give an *abnormal* animal psychology. The sad fact has been that it has always pushed forward these exceptions as the essential phenomena of animal mind. It has built up a general psychology from abnormal data. It is like an anatomy written from observations on dime-museum freaks.

Conclusion.

I do not think it is advisable here, at the close of this paper, to give a summary of its results. The paper itself is really only such a summary with the most important evidence, for the ex-

tent of territory covered and the need of brevity have prevented completeness in explanation or illustration. If the reader cares here, at the end, to have the broadest possible statement of our conclusions and will take the pains to supply the right meaning, we might say that our work has described a method, crude but promising, and has made the beginning of an exact estimate of just what associations, simple and compound, an animal can form, how quickly he forms them, and how long he retains them. It has described the method of formation, and, on the condition that our subjects were representative, has rejected reason, comparison or inference, perception of similarity, and imitation. It has denied the existence in animal consciousness of any important stock of free ideas or impulses, and so has denied that animal association is homologous with the association of human psychology. It has homologized it with a certain limited form of human association. It has proposed, as necessary steps in the evolution of human faculty, a vast increase in the number of associations, signs of which appear in the primates, and a freeing of the elements thereof into independent existence. It has given us an increased insight into various mental processes. It has convinced the writer, if not the reader, that the old speculations about what an animal could do, what it thought, and how what it thought grew into what human beings think, were a long way from the truth, and *not on the road to it*.

Finally, I wish to say that, although the changes proposed in the conception of mental development have been suggested somewhat fragmentarily and in various connections, that has not been done because I think them unimportant. On the contrary, I think them of the utmost importance. I believe that our best service has been to show that animal intellection is made up of a lot of specific connections, whose elements are restricted to them, and which subserve practical ends *directly*, and to homologize it with the intellection involved in such human associations as regulate the conduct of a man playing tennis. The fundamental phenomenon which I find presented in animal consciousness is one which can harden into inherited connections and reflexes, on the one hand, and thus connect naturally with a host of the phenomena of animal life; on the other hand, it

emphasizes the fact that our mental life has grown up as a mediation between stimulus and reaction. The old view of human consciousness is that it is built up out of elementary sensations, that very minute bits of consciousness come first and gradually get built up into the complex web. It looks for the beginnings of consciousness to *little* feelings. This our view abolishes and declares that the progress is not from little and simple to big and complicated, but from direct connections to indirect connections in which a stock of isolated elements plays a part, is from 'pure experience' or undifferentiated feelings, to discrimination, on the one hand, to generalizations, abstractions, on the other. If, as seems probable, the primates display a vast increase of associations, and a stock of free-swimming ideas, our view gives to the line of descent a meaning which it never could have so long as the question was the vague one of more or less 'intelligence.' It will, I hope, when supported by an investigation of the mental life of the primates and of the period in child life when these directly practical associations become overgrown by a rapid luxuriance of free ideas, show us the real history of the origin of human faculty. It turns out apparently that a modest study of the facts of association in animals has given us a working hypothesis for a comparative psychology.